NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON, D. C.

ORBITING ASTRONOMICAL OBSERVATORIES PROJECT BRIEFING

Auditorium
NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
1520 H Street, N. W.
Washington, D. C.

9:00 a.m.
Tuesday, December 1, 1959

DR. NANCY G. ROMAN, Head, Observational Astronomy Program, Office of Space Sciences, Presiding.

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9:00 a.m. 1 Dec 1959
Auditorium
National Aeronautics and Space Administration
1520 "H" Street, N. W.
Washington, D. C.

Presentations --

1. Basic Philosophy of the Orbiting Astronomical Observatories Project
   NASA Astronomy and Astrophysics Programs

2. Ultraviolet Stellar Spectrograph
   NASA Goddard Space Flight Center

3. Ultraviolet Photometer
   University of Wisconsin

4. Ultraviolet Sky Mapping
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   Princeton University

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Discussion Period

Preface

This transcript is an essentially unedited version of the proceedings of a meeting held to acquaint the industrial community with the technical aspects of the Orbiting Astronomical Observatories Project.

It should be understood that this was not a proposers, bidders, or manufacturers' conference, but purely a meeting whereby NASA could provide additional information to those companies which have shown a continuing interest in this project and particularly answer the numerous questions which have arisen.

For clarification of statements made at this meeting it is requested that you contact the speakers directly.

Nancy G. Roman

P R O C E E D I N G S

DR. SCHILLING: Good morning, ladies and gentlemen. It is my pleasure to welcome you to this briefing session on our orbiting astronomical observatories projects. Each of you has received our invitation and a tentative agenda. We are not ready yet to let out formal invitations for bidding on this project. Rather, we have experienced since the last few months that many of you have become so interested in this project that we have had with us just about everyone in this room, and I think many of you have visited some of our contractors who are merely engaged in preliminary instrumentation and scientific development.

We thought the easiest way for you, as well as for us, would be to present our present plans, and the present status of this project in a general session like this one. The Orbiting Astronomical Project is one of the projects in our observational astronomy program. The head of this observational astronomy program is Dr. Nancy Roman, who will lead this session today, and present to you the various speakers assembled at this table.

I want to introduce to you at this time Dr. Nancy Roman.

DR. ROMAN: Thank you. I think I had better stay seated. Astronomers have for a long time wanted to get beyond the earth's atmosphere. The earth's atmosphere has two very serious effects. First, the light which does get through the atmosphere is distorted, and secondly, much of the radiation does not get through because of molecular absorption in the earth's atmosphere.

Balloons and now satellites give us the first opportunity to get beyond the atmosphere and study radiation from celestial sources in an undistorted form. However, all astronomical sources with the exception of the sun are faint, and almost all, because they are very distant, are extremely small angularly. Therefore, you can not exploit the advantages of getting above the atmosphere unless you are able to get up there reasonably large sized telescopes, and unless you are able to keep these telescopes pointing at one region of the sky for long periods of time to a high degree of accuracy.

It was with these considerations in mind that we started the Orbiting Astronomical Observatories Project. The purpose of this project is to satisfy these requirements, that is, to get a telescope of moderate size, up to perhaps 36 or 40 inches in diameter, in a satellite, which can be pointed to within a fraction of a second of arc for periods as long as the star remains visible, which for the type of orbit we are considering is about 45 minutes to an hour in most positions.

Now, it is obviously a fairly large undertaking to produce such a satellite. It is also obvious that these basic requirements are necessary for almost any type of astronomical experiment which we wish to conduct in the region in which optics are used. Therefore, we have planned the satellite for this project with the idea that we would have a basic shell, if you want to call it that, which we have been calling our stabilized platform system, into which various types of optical instrumentation could be inserted. We have divided the responsibility on this project. NASA is directly taking over the responsibility for the stabilized platform system which will provide all of the features common to any astronomical experiment which we would like to do. This will include the stabilization, guidance, power supply, and some sort of finder system to tell where the telescope is pointing.

In addition to this, responsibility for individual experiments has been distributed among university and other scientific groups who are represented here today.

I think at this point I would like to go around the table and introduce the people whom you see here. Starting from this end, we have Dr. Code, Washburn Observatory, University of Wisconsin. Mr. Triplett, Ames Research Center, an NASA laboratory. Mr. Robert Davis, Smithsonian Astrophysical Observatory. Dr. Kupperian of Goddard Space Flight Center of the NASA. Mr. James Milligan, Goddard Space Flight Center. Dr. J. Rogerson, Princeton. Dr.

Liller of the University of Michigan Observatory. You will hear from each of these gentlemen in just a minute.

I think most of you have received the preliminary specifications which were prepared at the Ames Research Center outlining the general requirements for this project. If any of the companies here in the audience have not received these specifications, if you will please give me a slip of paper with your name and address, I will see to it that you get a copy within a week or so. I do not have any to distribute this morning, unfortunately.

Dr. Schilling has already outlined the purpose of this meeting. I think you all have the agenda in hand. The idea is that each of the astronomical groups represented will give a short description of their experiment. I will then invite questions from the audience on that particular experiment. At the end we will have a discussion of some of the engineering aspects of the project by the Ames Research Center, and then we will throw the meeting open to general questions on any portion of the project. I hope to be able to finish the major portion of the meeting by one o'clock, but if there is sufficient interest to continue into the afternoon, this will be possible.

There is one other point which perhaps I should mention at this time. The preliminary specifications were written in terms of the Vega vehicle. It appears now as if the Vega may be eliminated, and instead an Atlas Agena B substituted. This is a very new development. I don't think it will affect the program markedly. I have not had the time to go into the capabilities of the Agena in detail. This is a very new development, as I say, and it still is not firm. However, it looks like it should be able to do about the same job as the Vega.

With this I will turn the program over to the astronomers -- I will take it back. There is one other person in the audience I would like to introduce at this time. This is Mr. Curran of our Procurement Division. Mr. Curran, will you please stand? If any of you have detailed questions on procurement procedures, as compared to technical questions on this project, I would appreciate it if you would direct it to Mr. Curran.

Now I would like to turn the meeting over to Mr. Milligan, who will discuss the Goddard experiment for an ultraviolet stellar spectrograph.

ULTRAVIOLET STELLAR SPECTROGRAPH, NASA GODDARD SPACE FLIGHT CENTER, BY J. MILLIGAN

MR. MILLIGAN: I would like to spend a few minutes sketching out to you a tentative optical design for an astronomical telescope. I will spend very little time, if any, on techniques of guidance, or things of this sort. I will leave that for questions later on.

(Blackboard demonstration.) (See Figure)

MR. MILLIGAN: The telescope that we are envisioning will probably take the form of something of the sort I am sketching out on the board. At least it will be able to fit into the overall constraints that I am giving you. The telescope will consist, that is, the collecting optics will consist of a 36 inch mirror which will be operating probably around F-1, and a secondary which will put a return beam through the hole in the primary mirror of about F-5. Since this is a 36 inch mirror, this distance from here to here will be of the order of somewhere between 30 and 36 inches, something of that order of magnitude. The light will then come through a diaphragm here on a collimating mirror, back into the telescope, on to a large grating, on to a camera here, over to a detector system.

Now, this system is being designed for spectrometry. We are asking for moderate to low resolution. We are more interested in photometry than we are in high resolution work. We are talking in terms of the spectral resolutions varying from one angstrom, which will be our best resolution, to resolutions of the order of 50 angstroms. This telescope is being designed for two purposes. One, to get absolute energy distributions of stars in the wavelength regions from 4,000 angstroms to 1,000 angstroms. The second thing that we want to use it for, we want to get spectra on an absolute basis of emission nebulae which occur in the sky and integrated stellar systems, that is, galaxies. So we have essentially two different purposes. In one case, we are dealing with a point source in the sky. In the other case we are dealing with an extended region.

Our guidance accuracies in the two cases will be a little bit different, say from the point source here for the extended region, called No. 2. From the point source it looks as though we are going to need guidance accuracy of the order of one second of arc for periods of the order of 40 seconds. That is, we want the drift within a period of 40 seconds to be no greaterthan one second of arc. On the extended emission regions we will be very happy if we can get guiding accuracies of the order of one minute of arc for a period of, let us say, 25 minutes.

At the present time the plan is to use one or more photomultipliers as a detection system. These photomultipliers will have slits in the front of them, and each one of them will be looking at a certain wavelength band. To produce scanning of the spectrum in the observatory, it looks as though we are going to have to rotate this grating in finite steps every 20 seconds, something of this sort. That is one reason I gave guidance of one second accuracy in the period of the order of 40 seconds.

The detectors will be used as photon counters and the readout system will be something as follows. We want to have data of the order of one per cent accuracy. This is very, very difficult to do in a telemetry system such as we are probably going to be dealing with in a satellite. The way we are going to be able to do this, we want to be able to count these photons in each one of the detectors so that photon-wise we will have accuracies of the order of one per cent with a final accuracy of .1 per cent. The way we are planning on doing this at the present moment is to essentially count on each one of these detectors and measure the time interval it takes for us to count a certain number of counts.

We will then store the time it takes for the detector to count the counts, and telemeter the information back to the ground. So essentially what we will be telemetering back is not the number of counts that we receive, but the number of counts which we feel is necessary to produce the statistical accuracy that we need, and we are telemetering this information back in terms of a time system, that is telemetering back the time it takes to develop these numbers of counts.

DR. MEINEL (Kitt Peak National Observatory): That is only one per cent basic accuracy.

MR. MILLIGAN: I said to the order -- this number is a free-parameter at the present moment.

DR. MEINEL: Ten to the sixth?

MR. MILLIGAN: This number is only a free-parameter at the moment. This means that as we scan a stellar spectrum, we are going to have to have storage of our data. We feel that this is part of our experiment so that we will handle this ourselves. We will supply a data storage system and make it compatible with the telemetering system.

At the present moment it looks as though we are going to have to go to a core type memory instead of a tape recorder. As far as telemetry goes, it looks as though the basic experiment will be easily handled by two channels of information. One, for example, could be an FM, which is not at all definite, an FM system, let us say, with a 1 KC band width. The second one will be a lower frequency channel, let us say of the order of a tenth of a KC band, which we will commutate to read back information such as characteristics of the detectors, voltage, and things of that sort. That is, environment type devices.

As far as the accuracy goes, this is all the telemetering requirements we see at the moment other than the telemetering requirements which are required for the engineering aspects of the system.

To obtain the one second of arc guiding accuracy is somewhat of a problem right now. We are not at all sure whether our experiment has in it the capability of giving us an/error signal which will allow a guidance system, in the satellite, to point to one second of arc. There is a possibility of using the zero order image in the spectrum. We are looking into it. We do not have a solution at the present moment.

Now, exposure times, let us say on an individual star, for one angstrom resolution, the minimum exposure time will probably be of the order of five minutes. The maximum exposure time is probably of the order of 200 minutes for a star with one angstrom resolution. If we go down to our bandwidth of 50 angstroms resolution, the exposure times here are about the same. This is of the order of five minutes again, and this time up here has been cut down maybe a factor of ten, something like that, of the order of 20 minutes. So it will take between five minutes and 20 minutes for us to look at a particular object and get all the data back.

This presents a problem because it looks like an awful lot of objects can be gotten in this five minute period of time. So this means if you work only the five minutes of time to get the kind of statistics that we deem necessary, there will be a large part of the orbit over which our instruments may not be gathering data for us. Unless the instrument has the facility of being able to be programmed at least to a small amount in advance from the ground, that is, after a certain object is finished, we might be able to program the telescope to start finding some other object in the sky, something of this sort. There is one additional engineering thing we are going to need in the telescope. That is, we are going to have to have some way of disabling or keeping the system from looking at the sun. Now, this could be done, let us say, in a couple of different ways. One, we could put a shutter across the primary mirror such that when it got too close to the sun it would close. This is one possibility. Another possibility might be that one would have a photo cell system of some sort, which, if the light was above a certain amount, it would disable the system and move it away from the sun. How this is going to work at the present moment is mainly an engineering determination of what sort of system fits best into the overall capabilities of the system.

I think I had better stop right now. I shall be glad to answer any questions.

DR. ROMAN: Thank you. Before you do, I think there are people standing. I believe there are two or three seats over on this side of the auditorium if you would like to try to find them.

I would also request that for the sake of the reporter before you ask a question, you state your name and company.

Finally, one other announcement which I should have made earlier, and that is that there may be representatives of the press here.

Now, are there any questions on the Goddard experiment, the stellar spectrophotometer?

DR. HELVEY (Radiation Inc.): Just one question. You mentioned that data storage system will be furnished or will be manufactured in-house, at your place. In other words it would not be farmed out to industry. Is that right?

MR. MILLIGAN: No. Since we consider this part of the experiment essentially, we feel that since it is so crucial to our experiment that we will consider it part of our experimental apparatus and that the weight allocation probably and the authority or the blame will be placed upon the experimenter to have a workable system. I am not saying that we are going to build the system in-house. Obviously we don't have the capability.

DR. HELVEY: Yes, because we have special capabilities in this field. That is why I asked the question.

DR. ROMAN: I think perhaps the main distinction as to whether these things are the responsibility of the NASA directly or of the individual experimenters is that the contracts from the NASA part of the program will be let directly from headquarters here, whereas as far as the aspects which are involved with the individual experimenters, they will be handled by the experimenters.

DR. HELVEY: Thank you.

MR. HALLOCK (Gruman Aircraft Corporation): Can you elaborate on the grating distance?

MR. MILLIGAN: The grating will probably be a plain grating. It will probably be a 15,000 line grating of the order of 10 inches across.

MR. MITCHELL (Boeing Airplane Company): In talking about the guidance, the point source, you mentioned one second of arc, and then called it "drift". Is it absolute pointing accuracy or is it a rate which is more critical?

MR. MILLIGAN: The thing we want to be able to do is to hold the telescope pointing at a star within one second of arc for a period of, let us say, 40 seconds. Now, it can drift plus or minus that, plus or minus a half second of arc during those 40 seconds.

MR. MITCHELL: What is the absolute pointing accuracy you need to start with? The field of view I think was one degree.

MR. MILLIGAN: The field of view on this will be of the order [illegible] in arc, at least as far as our experiment goes, of the order of three seconds of arc.

DR. ROMAN: This means that the pointing will have to be within a few seconds of arc.

MR. MILLIGAN: And after we lock onto it, we will have to hold that accuracy plus or minus one half second of arc.

MR. KIRCHOCK: (Pierpont): Have you considered the use of a light funnel to reduce the accurate pointing requirement of the spectrometer?

MR. MILLIGAN: No, we have not. Actually, this project has only been under way a short period of time. We have a lot of design work left to do on our experiment, an awful lot of it.

DR. ROMAN: Am I right in thinking that a light funnel would not solve the problem because the problem is to exclude other regions of the sky? It is not a matter of funneling the light into a small region of the instrument.

Any other questions on this experiment?

MR. TRIPLETT: You mentioned one second of arc guidance for 40 seconds. Later on you spoke of exposure time of five minutes.

MR. MILLIGAN: Each sample will be taken on the order of 40 seconds. Then we will have to move the grating. It will have to come back on again and hold it for a given period of time.

QUESTION: The problem of the light funnel is not so much the extended region of the sky, but to maintain the collimation. You have to fill the collimator and nothing else.

DR. ROMAN: Any other questions or comments from the audience on this?

In that case we will proceed to item 3 on our agenda, the Ultraviolet Photometer, the University of Wisconsin, and I will ask Dr. Code to discuss this instrument.

ULTRAVIOLET PHOTOMETER, UNIVERSITY OF WISCONSIN, BY DR. A. D. CODE.

DR. CODE: Let me say something first about the astrophysical problems that we are concerned with. I think you will see that there is similarity in the problems that we are dealing with, and those of all the stellar experiments, that which was described by Dr. Kuperian and the Princeton program, in that we are interested in measuring the radiation from stellar objects and from gaseous nebula or interstellar gas in a restricted wavelength region.

At the University of Wisconsin for a number of years we have been concerned with two problems in which the extension to the ultraviolet is just a logical extension. One is the energy distribution of stars. The second is intensities of emission lines in gaseous nebula. What we really want to know are, let us say, the number of ergs per second per square centimeter per angstrom incident on the outside of the earth's atmosphere from a stellar source.

(Blackboard demonstration.)

DR. CODE: For example, if we plot the energy as a function of wavelength for a star similar to the sun, say, we can determine such an energy curve from one or two microns to .3 of a micron, and this pretty well includes all the energy distribution of the star. We have good leverage when comparing the energy distribution of the star with theory.

Let me extend this axis on down to the ultraviolet. For the hotter stars, say a star that is equally bright in the photographic region, we expect the energy distribution to look something like this. Most of the energy will be in the ultraviolet. In other words, we make observations of this part of the spectrum. We are not learning much of the stellar energy distribution. Therefore we are not able to fit theoretical computations for stellar atmospheres to the observations with any great certainty. At the present time we observe energy distributions with a band width of the order of 10 angstroms by a scanning spectrograph similar to that which Dr. Kuperian described, a rotating grating. We scan across the spectrum and derive such energy curves. An important feature, however, is that we have to have the instrument calibrated. We want to know what the sensitivity function of the whole instrument is, at any rate. It is not so important to know exactly how many ergs per square centimeter per second. We want to know that there is twice as much energy here as here. For terrestial observations, this is simple in a way. It is an extremely difficult proposition, but at least we can observe some standard radiation source, and then observe the stars.

For observations in the ultraviolet from a space vehicle, we are going to have to calibrate the instrument and then hope that this calibration remains or build in some way of checking the calibration. Now, there is one built in way for checking the calibration, and that is to keep going back to the same star. Similarly with band widths of the order of ten angstroms one can measure the intensity of the emission lines and diffuse nebula, and from these lines possibly determine the temperatures of the nebula, density, and possibly something about the space distribution of the gasses, too, the non-uniformity density of the distribution. But what we are not able to do in this case is separate very well the effects of different chemical compositions from effects of temperature and densities. There is some of the order of 100 emission lines that you would expect from the region of 1,000 angstroms to 3,000 angstroms that would give you a great deal of leverage on this problem. So we would like to carry out the same kind of observations we have been making into the region from 1,000 angstroms to 3,000 angstroms. Initially, we thought that a considerable amount be learned from wider band widths, a multicolor photometry. By wider band widths I mean delta lambda of the order of 100 angstroms, and that we could probably isolate these bandwidths with combinations of the spectral response, of detectors and filters. In such a photometer, then, to sketch it roughly we have managed perhaps getting four simultaneous measurements in four different wavelengths from the ultraviolet with four separate off-axis paraboloids.

We are looking down at this device. Here are the four mirrors and the focus for each one is brought off to the side here through diaphragms that we can change and filters that we can change to a small mirror with a field lens and project an image of the telescope objective on to a photomultiplier. Then this system would include both the acquisition of the star or the finding of the object and the measurements. The procedure would work something like this. When we are hunting for the star, we would incorporate a set of solar aspect cells that would tell us the angle between the optical axis of the telescope and the direction of the sun to, let us say, one degree. Then we know that the telescope is pointing somewhere in this one degree cone, scan around with two of these photomultipliers operating, with one degree diaphragms, one of them a yellow filter and one of them a blue filter. So if we get a deflection, this is a star somewhere in this one degree cone. We have measured the brightness in the yellow and the color, this yellow-blue base line, this is sufficient to tell us the name of the star. Then we would put in a diaphragm of the order of five minutes of arc, and attempt to center the star in this diaphragm and then we are ready to make the observations in which case all four cells are operating at four different ultraviolet bandwidths with 10 minute of arc diaphragms.

Now, if we wanted to observe early type hot stars with 10 inch mirrors and, let us say, something like 10 per cent efficiency for the optics and a 20 per cent quantum efficiency, as some kind of guess, with a 100 angstrom bandwidth and a star that is sixth magnitude photographic, we would be dealing with something of the order of 3,000 photoelectrons per second or a signal to noise ratio of the order of 50 to 60 for a measurement in one second. This of course is a guess. The purpose of the experiment is to really find out how good the guess of the number of events is. Therefore, since we really don't know how bright these objects would be, and we would like to work over a range of densities, we would like to have a dynamic range of the order of 10 to the sixth, and we would like to achieve a one per cent accuracy in the measurement.

Now, we have imagined perhaps doing it in this manner. First of all, while you are searching you would have to have continuous transmission of data so that as you pass over the star you know right then that you have the object. So that we have imagined some system of this sort.

Let us say here is one of the photometers. We would use this photometer to moderate an audiooscillator and actually two for each of the photometers; one that would give the gain of the amplifier involved in this photometer and another the counts. For example, suppose we present this in an analogue way. Here is a deflection from the star. If this deflection exceeds 80 per cent, then we change it again by a factor of ten. If the deflection drops back down to the order of 30 per cent, then the gain changes. That is, the gain is going to be constant over this range from 30 to 80 per cent. So we would give the gain of the amplifier and the deflection with that gain. Then this would be sent out with an FM transmission. When we are actually making the observations, with the oscillator, pulse shaker, we count the pulse and store, there are two binary counters for the gain, and the deflections, we can store these, perhaps with a tape recorder, or transmit the pulses directly.

It is some system like this that we imagined for the simple photometer in which case the band as far as the servo loop in finding the star, it means that the presentation of the data at the ground station has to be quite fast and efficient. We want to be able to make a decision right now in order to center the star in this diaphragm.

Now, the next logical extension is to narrow the bandwidths and a larger choice of bandwidths, which leads us to a spectrograph similar to that described by Dr. Kuperian. The one feature that I might point out is this. From the standpoint of, let us say, guidance for a given astronomical problem, spectrophotometric problem, a pertinent quantity is the ratio of the angular patch of the sky which you can take in with your entrance diaphragm or slit to the spectral region you can isolate, ratio of angular resolution to spectral resolution. We would like this number to be as large as possible. If for a given spectral purity we could take a very large patch of the sky, then as long as the sky background is not causing difficulty, it means that your pointing accuracy is not as high, the requirements for pointing are not as high providing you meet this ratio. This is just a function of the angular dispersion of the grating, the angular dispersion of the grating and the ratio of the diameter of the collimator to the diameter of the telescope. The bigger the bundle of light you take, that is the bigger the grating and collimator, the less accurately you have to point the telescope. The higher the angular disperson -- let us see, this is radians per angstrom, the higher the angular dispersion, the less accurately you have to point your telescope.

QUESTION: Do these backgrounds diffuse intensity or are they all point sources?

DR. CODE: These considerations would apply to both point source and nebula. Let us think of it for a moment -- we are pointing at a star, how wide can we make the slit, how wide angularly can we make the slit? Now we have to hold the star within this slit width.

DR. ROMAN: Could we have your name and affiliation?

MR. WHITNEY: My name is Whitney with Thompson, Ramo and Woolridge.

DR. CODE: The reason I mention this is that you are going to hear about requirements for angular resolution greater than one angstrom, a tenth of an angstrom or so. Thus, if we are going to have a bandwidth of ten angstroms, the pointing accuracy required is one hundred times less than it would be for any system requiring a tenth of an angstrom resolution.

This kind of spectrophotometry does not put as high a demand on pointing or also upon the length of time that one wants to store. It puts the same requirement onthe amount of data that one is handling as the higher resolution experiments you will hear about. So if you provide a stable platform that will take care of that, then this problem is adequately taken care of.

DR. ROMAN: I invite discussions of the Wisconsin experiment.

MR. FRIDGE: (United Aircraft Co.) I did not understand why you were using four mirrors which have to be aligned carefully and maintained that way for a long time. Why not a single mirror?

DR. CODE: Because in this case they do not have to be maintained to get them to focus in one place. Five minutes of arc is not a very serious requirement.

MR. FRIDGE: Will that maintain the spectral accuracy?

DR. CODE: The band width is being maintained by the transmission of filters and spectral response of the receiver. We are just trying to collect light in a pretty big hole. So it does not have severe requirements on the collimation of the optics. This way you introduce the least number of deflections.

MR. FRIDGE: Are the paths well separated?

DR. CODE: I don't know precisely how to isolate the spectral regions that I am interested in as yet. There are ways of doing it now. There may be better ways six months from now. They would overlap if we were to do it right now.

DR. ROMAN: Your aim is not to have them overlap, but you are not sure you can do that, is that correct?

DR. CODE: That is right.

MR. HARNED (Lockheed Missile & Space Division, Palo Alto, California): Do I understand you are identifying the stars by making a photographic measurement in terms of your color index and determining what star you are looking at from this?

DR. CODE: Plus the fact I know the angle between the sun and the star to one degree.

MR. HARNED: You have an initial one degree area. You can determint from this exactly the point you are on?

DR. CODE: Pardon?

MR. HARNED: You can determine exactly what star you are on, after you have separated out the one degree region?

DR. CODE: I know the star is somewhere in this band one degree wide. Then I know the brightness of the star and its color. If the star is bright enough this is quite unambiguous.

DR. ROMAN: Any further questions?

DR. KUPERIAN: Could I ask you what dynamic range your telemetering data has?

DR. CODE: Well, ten to the sixth is what we wanted to cover. But we have counters that get us, well, sufficient data for ten per cent accuracy actually. I talked about two channels, one for the gain of the amplifiers and one for the counts. These were a 10 stage binary counter and a four stage binary counter, a four stage binary counter that records the gain, and a 10 stage binary counter for the deflections. That is more than adequate for dynamic range of ten to the sixth and one per cent accuracy.

DR. KUPERIAN: That is one per cent everywhere for ten to the sixth accuracy?

DR. CODE: That is right.

DR. KUPERIAN: This data is simultaneous on four channels?

DR.CODE: That is right.

DR. KUPERIAN: And it could be stored?

DR. CODE: It can be stored. We had imagined that this system adds considerable redundancy, too, Perhaps one of these channels fails. You have three of them or two of them working. Perhaps the storage system fails. You can still get the data pulses directly, or, let us imagine that the guidance goes haywire, and you really don't know where you are pointing; you can put this on this continuous data transmission that you use to find the star. Then you just sweep the sky and you have to untangle the results you get but you can get some results.

DR. KUPERIAN: One more question in this connection. I am trying to bring out some point that it tends to generalize the system rather than be specific. Your pulse counting here, in other words, you have an event you wish to totalize in some instance.

DR. CODE: That is right. We want to code in some way. We don't want to send down every pluse.

DR. KUPERIAN: In other words, it is digital data or pulse data.

DR. CODE: You start off with a DC amplifier, a current device, and then convert it to digital in the scheme that we have imagined, and digital perhaps as much for getting through the noise and getting the signal down.

DR. KUPERIAN: You have some sort of current integrator that puts out a pulse after you have integrated so much. You speak of DC amplifying equipment.

DR. CODE: There is a blocking oscillator whose pulse rate is determined by a voltage output from this 100 per cent feedback DC amplifier.

DR. KUPERIAN: So it is not a tight current integration.

DR. CODE: That is right.

DR. FOSTER (ARC): I have a couple of questions. You intend using the same detector for the UV as for the red, rather blue and yellow identification?

DR. CODE: Two of these photomultipliers would be the photomultipliers in a lithium fluoride bottle, and they would not go as far to the ultraviolet as the other two. Two detectors are capable of observing in the photographic and visual region. They are not capable of going as far to the ultraviolet. So these two channels are things like 2500 angstroms for the ultraviolet and 1800 angstroms, something of that sort.

DR. FOSTER: I have one other question. Then you apparently intend scanning in the sense of this range one degree wide, and as that slowly goes across you will look at the yellow and blue intensities to decide whether you want to stop at that one or not?

DR. CODE: That is right. Actually, one would have two speeds, a slewing and a setting. You go on past, you get a deflection, you come back with a setting and try to get the star in a hole.

DR. ROMAN: How slowly do you think your slew speed would have to be in order that you get the deflection?

DR. CODE: This depends on how faint a star you are going to observe. It would probably -- well, about a half a radian per minute would allow you to locate the second magnitude stars.

MR. BOLLING (Chance Vought Inc.) Dr. Milligan mentioned that storage would be his responsibility in the experiment. Dr. Code, have you thought of the same responsibility on storage?

DR. CODE: It is certainly an integral part of the experiment. This would be part of the package that includes the optics.

MR. BOLLING: On the data transmission, have you considered the "bits" per second, or the band width for your real time transmission?

DR. CODE: Occasionally I have come up with such numbers. This does not seem to be a very critical thing from our standpoint, namely, how many "bits" can you handle. This is just determined by how long we have to work on one star.

DR. ROMAN: Coming back to this question of on board storage being the responsibility of the experimenter, I think this is something we are going to have to work out. Almost all of the experiments will require on board storage. I think that we will probably try to develop a single system that everyone can use in which case it would no longer be the sole responsibility of the experimenter. On the other hand, the experimenter is certainly going to have to have appreciable input into what this system will be. I think from the standpoint of the contractors this is not an important question at this moment, since it probably will be contracted out regardless of who does the contracting.

Are there other questions?

MR. COOK (Space Technology Labs): I would like to know why the figure of 1,000 angstroms was decided on as the lower wavelength limit, that is, aren't there hot stars that have an energy below 1,000 angstroms which would be of interest? Also, would there not be spectral lines in this region from stars of interest which could be observed with a spectrograph?

DR. CODE: Actually this point here was supposed to be about 900 angstroms. At the Lyman limit 912 we expect the energy distribution of stars to just drop off completely. We expect the interstellar medium to be quite opaque. So we don't expect to find too much. But just because of this prediction, we ought to try it. That is why one of the bandwidths is there. We really would be surprised to find a great deal of radiation beyond the Lyman limit 912.

DR. ROMAN: Do you have anything to add?

MR. MILLIGAN: There is also the additional problem, as some of you may know the reflectivity problem in the ultraviolet is quite serious. At the present moment the present state of the art is such that if we are going in the region below 1100 angstroms, your reflection falls off a very good mirror drops off to about 10 per cent. In fact, you are very lucky if you can get 10 per cent. This coupled with the fact that if you want to fly a window detector, and lithium fluoride looks like the best one you can use, it has a crystal cutoff of 1050 angstroms, so the combination of the reflectivity, the interstellar absorption, things of that sort, and the detector problem tend to limit us a little bit.

DR. ROMAN: The next question?

MR. ANSELM (Beech Aircraft): In regard to the portion of the experiment on nebular gas mass are you interested in verification of gas constants? My reference would be in regard to the determination of speed of sound at very, very high altitude.

DR. CODE: I don't quite understand. In the gasseous nebula you are referring to?

MR. ANSELM: Yes.

DR. CODE: This of course is a feature -- I don't know of an experiment -- we think we know in the gas nebula what the speed of sound is. We can measure it by the number of electrons.

MR. ANSELM: Do you have verification of it?

UNIDENTIFIED: We are doing some work at the present time on the accoustics.

DR. CODE: It is possible to tell whether the excitation of spectral lines is due to radiation or collisional excitation. This it would be intended to explore.

DR. ROMAN: The next question.

MR. McCLOSKEY (Aerojet General Corporation): Will a copy of this be made available so that we don't have to keep notes?

DR. ROMAN: I think it would be safer for you to keep notes on the areas in which you are interested. I doubt that we will be able to make the entire proceedings available to everyone.

Are there any other questions? If not, we will pass on to the Ultraviolet Sky Mapping experiment, by the Smithsonian Astrophysical Observatory. Mr. Davis.

ULTRAVIOLET SKY MAPPING, SMITHSONIAN ASTROPHYSICAL OBSERVATORY, BY B. DAVIS.

MR. DAVIS: First, the reason why there were so few of the preliminary specification sheets brought down here was that it still was a rough draft, and we did not want too many obsolete copies drifting around three or four weeks from now when we get that whipped into shape. Anybody that does want the smooth version of this rough draft and possibly extra copies of the drawings that went with it should write to us. I think the address is on here.

DR. ROMAN: Perhaps you had better read it, since the ones who do not have the copy will not have the answer.

MR. DAVIS: Smithsonian Astrophysical Observatory, Cambridge 38, Massachusetts. We will send out copies when they are ready.

The information we are interested in is the same as Dr. Code and Dr. Milligan are interested in, the spectral intensity distribution of stars and interstellar matter.

There are approximately 10 million stars brighter than magnitude 15 photographic.

DR. ROMAN: For the sake of the non-astronomers we might add that this is roughly 10,000 times fainter than can be seen with a naked eye, a little less.

MR. DAVIS: Yes. It is roughly the limiting magnitude of a 20 inch telescope with good photographic film in a few seconds exposure time.

These stars have a temperature distribution -- number of stars versus temperature with 50,000 degrees up here. The sun being about 5,000 degrees, the lower end we don't know too much about yet. But the numbers are roughly like that. The hotter stars, there are very few, probably it falls off even more rapidly than this.

(Blackboard demonstration)

MR. DAVIS: Of course, when you get down to zero temperatures, there are zero stars, but it is fairly flat in the area from about 10,000 degrees to 3,000 degrees, roughly equal number of stars at all temperatures. Then these really hot objects where most of the ultraviolet comes out, where most of the light comes in the ultraviolet, there are very limited numbers. When we start pointing telescopes at the sky, that are sensitive to ultraviolet, we start picking up mainly the hot stars and not so much the cooler ones. For instance, we may have a telescope with a two degree field of view sensitivity in the ultraviolet, and out of the ten or so stars that may appear in that field that are sensitive enough to be picked up down to the 15th magnitude photographically, the hotter stars, about nine out of ten of these will be down in this distribution tail of hot stars. Only one of ten or so will be as cool as Sirius, 10,000 degrees, and practically none will turn out to be as cool as the sun if present theories of astrophysics are correct.

What we want to do is take a look at every star in the sky bright enough to show up in our system, see what it does look like, or at least refer to the fact that it is there. In order to do this in a reasonable amount of time we can't use diaphragms and photocells. We have to use either television or photography. For many experimental reasons we have decided that television is the answer to our problem. Taking photographs, or pictures, rather, of fields of the sky about two degrees square, there are roughly 40,000 square degrees in the skies, so this means 10,000 pictures in which color our equipment is sensitive. In this way, whether astrophysicists think stars are interesting or not, at least we will have it and we will know whether it is interesting. About 90 per cent of our stars will be hotter than 10,000 degrees. Hopefully, if everything goes right there will be a few of the brighter stars coming through that are down in the cooler temperatures where most of the radiation does come out, where we can see it, but interesting things may happen in the ultraviolet.

Taking this special distribution curve again, for the sun the curve is something like that. For a hotter star such as Sirius, the curve will be more like that. We can observe down to about 3,000 angstroms. These curves are not quite drawn in right. In the case of the sun, we have continued the curve on down below 3,000 to the extremely short wave lengths. In the case of the bright stars, there is at present no observational information below this point.

Now, by these means we can trace this curve by putting three points on it, or with spectroscopic resolution we can obtain the full curve. Television techniques will put on the three points at 2600 angstroms, 1900 and 1350. In addition, since television does not give really accurate magnitudes, we are hoping for ten per cent over a dynamic range, possibly split up into separate ranges from the nature of the beast, a total dynamic range of 10 to the fourth, accurate to 10 per cent. If we were to add a photoelectric device we could get down to one per cent accuracy, and we do see putting an extra telescope into the platform with five minutes of arc diaphragm to isolate one star near the center of the field and look at it more carefully in six rather than three spectral bands. Each of these three bands will be taken care of by its own special telescope designed for sensitivity within a few hundred angstroms of the particular band. So together with the photoelectric device that adds up to four telescopes all of them identical, with reflectors eight inches in diameter,20 inch focal length.

We also foresee adding a fifth telescope with an objective prism, no slit involved in this device. It merely takes the same field and spreads the brighter of these stars into spectra. This one will probably be the brightest star in this field. It will be pre-selected for that. So that maybe three stars out of -- well, actually it would be close to three stars out of a hundred-- we would get slight spectra for it, and thereby cover the whole sky, getting, with the sensitivites we perceive, possibly as many as a million stars in the multicolor pictures and about 20,000 stars with slight spectra. The exposures required to obtain these sensitivities are ten seconds long. If everything goes right, exposures of one second will be possible, but we think we need ten seconds exposure to obtain adequate sensitivity. In order to separate close double stars, we want one minute of arc resolution. The television image tube is already been developed by Westinghouse Research Laboratory in Pittsburgh, and we have developed the optical system, it is already under construction. So the first two components of the block diaphragm are pretty much under control.

The aberrations and the image tube are matched to give one minute of arc resolving power. The remaining requirement is that the instrument be stable enough for ten seconds of time so that these images do not blur and that we know where we are pointing. We think both of these requirements are fairly well met. Then if this diaphragm is to be centered around a star, we have an additional requirement that we be able to determine by a pre-program device either from the ground or some other way, within five minutes of arc pointing accuracy. If we have that, it is very easy to find out exactly where it was pointing by looking at these pictures afterwards.

One possible method is a finder telescope with television to send back about a 15 degree field of the sky with sensitivity the same as we have on the earth and compare it with star maps. If we know within one degree where this is by astrophysical theory, we can identify these stars and measure it to one minute of arc.

Then the next step in our system is the TV camera. We are fairly certain now on what we need for a TV camera. We have done no design work. Some preliminary feelers have gone out, that is all so far, on a television camera to drive the image tube. But looking at the amount of information, each square minute of arc in a two degree field has ten possible bits of information for the brightness of the object in that particular space. Since we do not know that we are looking at stars we cannot use pre-recording, because we are also interested in what the nebulosities are like. Of course, with the slit spectra the object is spread out into lines. Or if they are small nebulae we might even get a stream of points representing its emission lines. There are 144,000 more or less in this size field, 144,000 -- this thing comes out different this time than the last time I figured it out -- there are something like 250,000 bits of information contained im one of these fields, so that storage in the satellite is something we have not contemplated. We plan to use live television throughout, controlling it from the ground when it is within view of the station and just letting it sit there for the other 90 per cent of the time at as low a power drain as is feasible. In other words, merely have some kind of radio receiver on so that we can command the transmitter the next time around.

Using this method it would take about four months to cover the entire sky. The amount of information involved is very high. We need a scanning rate of one frame per second. We need a 150 kilocycle band path merely to transmit the picture to the ground. We will need to store this information on the ground in a method that is amenable to automatic computing so that the astronomer does not have to do the whole thing with pencil and paper. But in the satellite we will/not store it. From here it goes into the telemetry which is the responsibility of NASA and the working group, until it gets back down to the ground storage again where it comes back to us.

The mechanical end of this to hold the optics together rigidly has not been worked out in detail. The drawings show how far it is. We know the dimensions. We do not know what type of hardware we will be using to hold it together. All the moving parts that we think we will need are shutters to close off from the sun if it comes too close. Everything else will be done by electron standards.

We also have in mind some spectroscopic slits spectroscopy similar to what some of the other institutions have mentioned, but have not gone to the design of additional instruments up to this time.

I shall try to answer any questions you have.

MR. DEL DUCA (Ramo-Wooldridge, Inc.): This looks like a lot of power. Have you any idea of the order of magnitude of the power? Two, and this applies to all the rest of the presentations, is the experimenter going to provide any secondary source of power in addition to the primary vehicle power? Third, what kind of auxiliary or secondary power are you thinking about if you are thinking about it.

MR. DAVIS: In the first place, most of our power requirements are motion of the platform, the heaters for the television tubes, and the transmitter power for the telemetry. These are roughly equal and probably in the vicinity of 10 watts each for 20 to 60 minutes per day. I do not think this requires additional power beyond that that is anticipated for the satellite.

DR. KUPERIAN: How much is that part without guidance?

MR. DAVIS: Taking off this?

DR. KUPERIAN: Yes, the platform part.

MR. DAVIS: Just these three areas, almost all the power goes into the television heater which totals something like ten watts.

DR. ROMAN: Does anyone else envisage power requirements appreciably in excess of these?

MR. DAVIS: The transmitter power depends very greatly on what we have at the ground receiver. We were figuring this on 60 foot dish antennas. If the antenna on the ground is smaller, we need more power upstairs.

MR. DEL DUCA (Ramo-Wooldridge, Inc.): In case the vehicle power, main power supply, goes out so that you don't have anything for attitude control or guidance, you would like to just get what you can out of the experiment, do you have a secondary source contemplated?

MR. DAVIS: We would like to put a nuclear power source in. We are not sure we will be allowed to.

DR. ROMAN: Are you sure your cathodes would like it if you were?

MR. DAVIS: Yes.

MR. CHENG (Hughes Aircraft Company): Mr. Davis, your slewing time between frames, I recall faintly the last time I met you, would be something of the order of 10 seconds.

MR. DAVIS: Yes.

MR. CHENG: Does that still hold?

MR. DAVIS: That is what we are aiming for.

MR. CHENG: Could you possibly allow a longer time maybe lengthening the experiment somewhat?

MR. DAVIS: If you double that time you multiply the total four months by one and a half and end up with six months, we would still be happy.

MR. JOHN LINDSEY: I was wondering if you have looked into the possibility of storage using two systems -- frankly I don't know whether the resolution is adequate or not -- but one a Vidicon where you store it temporarily on the photoconductor, and second, a static electricity type recall which is being developed for TV work.

MR. DAVIS: We have been talking with RCA about this. They say it looks hopeful. This would reduce the four months that we need. It would cause a band width difficulty getting the information out in time when it is over the station. We would be happy to have it but it is not necessary for the experiment.

DR. ROMAN: Along the same lines have you considered photographic storage? Obviously you can't use photographic film with the idea of recovery, but what about storage of photographic film and scanning?

MR. DAVIS: We have considered this. If such a device could be made "fail-safe" we would enjoy very much having it. By fail-safe, I mean when you run out of film you can still use the machine, or if the film jams, you can still use the machine. If we can get these two points we would like very much to have it. Again it is not necessary, but it would help the experiment.

MR. LINDSEY: I have one thing which actually does not have anything to do with this experiment. The Space Technology Laboratory has asked why cut off at 900 angstroms. I am wondering if the answer to this is not really expediency? We know in the case of the sun that Rense, Colorado, Tousey and Hinterreger, AFCRC, have been working with the hydrogen 304 line. I think the people here might like to know if they could do it. I really don't know what the energy transmission might be, but I think the people in the future might be interested in looking at the short wave length, is that not true?

MR. DAVIS: In regard to the short wave lines from the sun they put an amazing amount of energy on the earth from the sun, but you take the next nearest star, Alpha Centauri, there are roughly four lines that we could pick up with this type of sensitivity. You take stars that we know nothing about, and we can't even make guesses. The theory of cool stars in the ultraviolet is pretty close to zero so that we can't make really educated guesses about how these stars will look. Yes, we want to look at them as far down as we can get.

DR. ROMAN: Dr. Savedoff, have you gone far enough in your work, in the far ultraviolet, to have comments on this question?

DR. SAVEDOFF (University of Rochester): We have not gotten very much further than we have been. We have been very much encouraged in using ordinary techniques, spectrographs and filters. The 304 line is still very much shaded by the interstellar hydrogen. It looks like the universe opens up again somewhere at a hundred angstroms and lower. We are very discouraged. When you think of working at one angstrom, you can use the platinum reflector. You have to be within one degree of the plane of the mirror in order to get reflection. That is a horrible requirement.

DR. DAVIS: We are looking into the use of zone plates, also. They start getting very difficult to make when you get below 50 angstroms, but we have hopes to use them . We have hopes of having zone plates in the near future that are good down to possibly 50 angstroms for focusing that way.

DR. LILLER (University of Michigan): I might just say that Professor Lawrence Aller at the University of Michigan has calculated some of the effects of the interstellar medium, and I think he is in agreement with Dr. Savedoff with one hydrogen atom in a cubic centimeter you get quite a number -- from 912 angstroms, and the Lyman wave limit to shorter wave length down to 100 angstroms range, and below, everything is pretty well blocked out. Below 100 angstroms you will start to see something. But between 100 and 1,000 angstroms, space should be pretty black.

MR. LINDSEY: That sounds like the answer still is that we are interested.

DR. ROMAN: But it is difficult to do.

MR. STEIN (Republic Aviation Corporation): Are you definitely ruling out the idea of recovery of information by the capsule technique?

DR. ROMAN: My feeling is that it is going to be extremely difficult to oonsider recovery of anything from such a vehicle without (a) changing its orbit, and therefore destroying its future usefulness, and (b) changing the balance of the instrument, so that even if itt could be used it would be extremely difficult to rebalance it to within a part of ten to the third or a part of ten to the fourth.

Any other questions? I think then we will go to the last of the stellar experiments, and then we will have a recess after that. I will call on Dr. Rogerson of Princeton University to discuss a high dispersion stellar spectrograph.

HIGH DISPERSION STELLAR SPECTROGRAPH,
PRINCETON UNIVERSITY, BY DR. J. ROGERSON

DR. ROGERSON: Princeton University Observatory is interested in investigating primarily the dark gasseous matter that exists between stars. The best way that we now know how to do it is roughly the following.

Fortunately we have some very bright light sources scattered around space.

(Blackboard demonstration)

DR. ROGERSON: If this now represents a cloud, an accumulation of dark gasseous matter, which being dark we can not see of itself, we can detect its presence by investigating the light of that star after it has passed through the gas cloud. So this now represents our satellite. We will look primarily at very bright, very hot stars, and investigate the effect of these interstellar gas clouds on that light.

It so happens that the gas cloud has considerable effect except that the effect is restricted to quite narrow wavelength regions. If I draw you once more a bit of a star spectrum in the ultraviolet, this is lambda, and this is energy. Hopefully we might have a very hot star that has no lines of its own. Let me take that for the moment. We have some distribution of energy. What the cloud will do then is subtract a rather narrow line, in general much narrower than any lines that the star itself would have in its own atmosphere.

Now, I bring this point up because it means that we must be able to resolve the spectrum well enough to see this. Hence our experiment is concerned with high dispersion spectroscopy. So our experiment then is to look at these very bright stars, disperse the lights in a spectrograph, and in some manner to detect the presence or absence, as the case may be, of lines which we have pre-selected. That is from atomic structure, molecular structure, we predict at what point of the spectrum these lines should exist. So we do not have to scan the entire spectrum.

These lines, if we can say something about them in angstroms to give you an idea to compare them with the previous presentation, the width of these lines is perhaps of the order of a tenth of an angstrom in width, which is I think something like a full factor of ten over the narrowest bandwidth that we had before mentioned. That means that in order to detect this absorption in the starlight, we must be able to resolve roughly the same band path, that is a tenth of an angstrom.

So much for what we are trying to do. Let me show you a little bit of what our present thoughts are on how we would go about doing it, what the equipment is.

We have a little bit of a problem in that we need very bright sources and, as was brought out before, these bright sources are rather scarce. When I say bright sources I mean intrinsically bright sources, that is if you were up close to them, you could see they were bright. They might be very distant. By bright sources, I am talking of the fact they are bright in their ultraviolet. They have a larger part of their total energy in the ultraviolet than in the visible. For example, because of this requirement, there are not too many of those stars, and in order to have a useful instrument, we have to be able to use these stars even though they are quite far away from us, and apparently rather faint.

The upshot of all this is that we need a rather large apperture, not the largest presented today, but the second largest; 24 inches is our apperture. This is a 24 inch diameter light collector apperture. The "F" ratio of this mirror is, according to a light design, F/3. There will be a secondary mirror, Cassegrain secondary, which will change the overall "F" ratio to F/20. (See figure)

The light then is brought to a focus at, let us say, this point here on a slit with a spectrograph. The entrance slit of the spectrograph is of the order of five microns, very narrow. You can already see what sort of restriction this is going to make on pointing accuracy if we are not going to have this wander on and off the slit.

Located down here roughly in the center of the grating will be a concave grating. The whole spectrograph will be a Rowland type of spectrograph. The light will come down and hit the concave grating and form the spectrum along the edge of the Rowland circle. Along the edge behind exit slits of the same width we will have a series of photo tubes.

I might explain why we have so many. I have four here. There is actually a fifth one, but that is only for a minor procedure early in the orbiting of the satellite and I won't go into that at the moment. We have, instead of four separate tubes, that is for four separate wave lengths, we have two pairs. Two will be for what we call the far ultraviolet, which may go down as far as 800 angstroms and may be up to 1600 angstroms. The other will go and take over at 15 to 16 hundred angstroms and go to 3200 angstroms.

Why do we have two? It comes back to the problem, how well can this thing be guided, once it is up there? It is quite possible that perfect guiding is impossible. Ours will oscillate back and forth with some amplitude according to the sensitivity of the guiding and it will cause a fluctuation of the reflected light. That fluctuation we are not interested in. In fact, if we get that fluctuation, it will distort the information going to one tube. What we are now thinking of is having two tubes and only on the ground taking the ratio of the output of those two tubes so that as the light intensity is fluctuating as it comes to the angstrom slit of the spectrograph it will affect both tubes by the same factor. If you take the ratio that factor cancels out.

That system is not new. It has been used several times already. So that explains the two pairs of tubes.

Now, in each pair we will have one which is held at a fixed wave length, and the other will scan by that spectral line which I have drawn previously. So that now as we take the ratio we hope there will be a dip in the ratio corresponding to the dip in that spectral line as the slit scans by it and that dip will be a real dip which is in the spectrum, and not a dip because the star happens to be momentarily off the entrance slit. We plan all of this to be pulse counting equipment, the output of all of these tubes. It is not certain yet whether we will try to send down to the earth the accumulated amount of counts in a fixed time interval, or whether, as Dr. Milligan's experiment mentioned, we might count up to some standard number of counts, 10,000 or 100,000, a standard number, and then simply telemeter down the accumulated time that it took to gather or accumulate that number of counts. But the time I think would be digitized information also for the same problem, trying to get down the information as accurately as possible.

Now, this is our experiment. If I may take the liberty of going a little beyond our experiment into the satellite as a whole, because as we have been thinking of it, we have not been able to disassociate in our mind the requirements of the satellite from the requirements of our experiment.

DR. ROMAN: I was going to suggest that you do this rather briefly, because time is running on.

DR. ROGERSON: All right. Well, it is very important for us to know what star we are looking at. Even though in Dr. Code's experiment he can get an idea of what the star is from the color and the intensity with practically no ambiguity, we would like none at all. So we would like very much to have essentially a finder telescope, a telescope backed up by a television which would only be on as the satellite passed overhead, and would be able to give us a 10 by 10 degree field. We feel this is ample for recognition of constellation patterns so that we can pick our stars.

At the time that we find that, we hope by direct command to the satellite to be able to move the selected star into the field of our instrument.

Now, this is a rather large field to get this aimed properly so we hope then to have a second television system. There will be a mirror at this point in our design with a hole in it, the total field being of the order of a degree, perhaps a little less. This light would then come out and feed into another television, the fine scale television, and the field here would show a hole. What we would then attempt to do, as soon as we see our star come into this field, we would then only look at this television and force that star into the hole. Once it was in the hole, then it would fall on the slit, and on the guiding mechanism, that is the guiding center, which is at this point here. That is, you may have, for example, the slit just tilted so that light may be taken off to the side into photomultipliers and give you an error signal. So once it goes through the hole, it falls on the error sensing device, and it comes back to you people to do something about it, and then bring the image back.

MR. WHITNEY (Ramo-Wooldridge, Inc.): We have not been able to establish the angular pointing control you need without the diameter of the secondary mirror.

DR. ROGERSON: Let me say we want zero point one second of arc (0.1) for essentially an indefinite period. We expect that the satellite will not be available to us except once every 12 hours, for example. We were thinking of a polar orbit. During that 12 hour period we would be on a star and observe the entire time. We hope to have a command memory which will tell these photo tubes what wave length to go to and then set them in an automatic scanning operation. Once that scanning operation had been completed several times, according to the accuracy that we are interested in, then it would move on to the next line. So the telescope would be in operation all the time except during setting on a new star. That information would be stored and disgorged when the satellite came overhead. The storage capacity is not great for this type of work, being something of the order of 10 to the fifth bits in a 12 hour period.

Now I would like to go further, but I think I am running over. So perhaps we had better have questions.

DR. ROMAN: I would like to make one remark first on the polar orbit, that is, have you thought of the fact that you are going to have to go through that Van Allen belt in the polar orbit?

DR. ROGERSON: Well, it has come up. We have not decided how bad that would be for it.

DR. ROMAN: I have spent most of the time thinking we want to stay away from the polar orbit for that reason.

DR. TRAGESER (Massachusetts Institute of Technology): Does your equipment have any moving parts in scanning this while you are moving, while making this alignment?

DR. ROGERSON: Yes. Let me say this. During the scanning we scan slit width by slit width. It is not a continuous motion. The motion stops. We integrate for whatever time it takes to get 10,000counts, for example.

DR. TRAGESER: How long does it stop?

MR. ROGERSON: Of the order of, say, five minutes. After it has accumulated those counts, those counts will go into storage. Then the slit will move to the next band width and scan again. So this is not continuous motion during the integration, but step by step. The steps are equivalent to five microns being in the spectral range equivalent to about a 20th and a 10th of an angstrom.

MR. STAROS (Sperry Gyro Company): Dr. Rogerson, do I get the impression from what you say that more than one experiment is performed in a single satellite, yours with somebody else's, for example, or would one satellite be used?

DR. ROGERSON: I don't think that is for me to answer. There has been a fair amount of discussion along those lines.

DR. ROMAN: I think the way the program looks now the answer is yes, that there will be more than one experiment in the satellite. That goes for all the experiments which have been discussed. This is a point I was going to bring up later. It is just as well to get it on the record now. It does not look feasible, with the number of vehicles that we have available or expect to have available for this program, to assign a single experiment per vehicle and still provide any leeway for backup in case of failure.

MR. KERPECHAR (Kearfott Co., Clifton, N. J.): Assuming you are interested in a high pointing accuracy, what influence does the aberration of light from the star, due to the change of velocity in orbiting around the earth, what influence does that have on your experiment?

DR. ROGERSON: As far as the pointing is concerned, I hope the guiding will be sufficient to take that out. But there are doppler shifts which will amount to the order of one of our slit widths maximum. If when we store our counts we also store the time so that we know where we are in the orbit, we can take account of that fact.

MR. KERPECHAR: You can get five seconds displacement on a selected star if you are moving 25,000 feet per second. Is your accuracy of pointing one second for 45 minutes, something like that?

DR. ROGERSON: Twelve hours.

MR. KERPECHAR: What is your integration time? Due to displacement on the sphere of the star, how far would the star move in an integration period? What is the integration time?

DR. ROGERSON: The integration time is of the order of five minutes, I would say. I hope that the guiding is going to be active so that if the image does tend to move, there is a correction, we don't set it at one point and leave the satellite to its own devices.

DR. ROMAN: What you are reallying saying is that the fine guiding has to have sufficient capability to take out a little greater than a second of arc error due to aberration.

DR. ROGERSON: We are hoping that we can keep within that.

MR. TRIPLETT: The guidance system has to track a moving target. It means that its maximum rate of motion is something of the order of three seconds of an arc.

DR. KUPPERIAN: Are we talking about the parallax?

DR. ROMAN: No, aberration.

MR. MEYER (Martin Company - Space Flight Division): Can you tell me how long your five micron entrance slit is?

DR. ROGERSON: The accuracy in that coordinate is not nearly as severe as/the slit. It can be several millimeters long, I would say, instead of the five microns or so, at least in the one coordinate.

across

QUESTION: I have a question concerning experiment management.

DR. ROMAN: Could we leave that for a little later? I would like these discussions to relate directly to the experiment. There will be time for the other type of questions. Any more on the experiment?

MR. MITCHELL (Boeing Airplane Company): What is the resolution you can get from your secondary mirror for guiding purposes?

DR. ROGERSON: I am sorry, I don't understand.

MR. MITCHELL: You indicated from the experiment you would provide an error signal to the guidance. What sort of resolution can you give?

DR. ROGERSON: We are assuming that we can give you a suitable error signal corresponding to a tenth of a second of arc.

MR. MITCHELL: It has to be somewhat better than a tenth of a second if you are going to guide that.

DR. ROGERSON: What I meant is so you can guide within this accuracy. I don't know whether that is so. At Princeton University we have a balloon telescope project which will be going out there next year -- excuse me, 1961. That is designed to meet these specifics, ,to guide within a tenth of a second of arc. I am hoping we will get practical experience on just how that can be done satisfactorily. At present it is a tough job but it does not seem to be impossible to provide an error signal that will allow a good servo mechanism to keep this pointing within a tenth of a second of arc.

MR. CHAMBERLAIN (Hughes Aircraft Company): What is the size of your grating?

DR. ROGERSON: It will be something like two inches square.

DR. ROMAN: Any further questions?

MR. HYDE (American Optical Company): We are working with fiber optics configurations. We are able to take a bundle of these optical fibers which is round at one end and rearrange the fibers so that they are in a straight line at the other end. Now, the positioning of the star has yet to be within the area of the round bundle at the incident end, but as the star wanders on this round bundle it comes out of fibers which are up or down in different parts of the entrance slit. By this method one can trade a tight tolerance in one dimension and a loose one in the other dimension for an intermediate tolerance in the two dimensions. Now we don't have fibers that work down in the far ultraviolet. We can't do this at this time. But this might allow you to compromise from this very severe tracking requirement you have, because of your requirement for very high resolving power.

DR. ROGERSON: I would consider the fact that the bundles are probably not transparent rather serious.

MR. HYDE: All I am saying is that we have not made fibers out of the materials that you have to use in this kind of system. We don't have any different requirements than you do in other places for providing transparent materials.

DR. ROGERSON: That is entirely true.

DR. ROMAN: I think this is a good point to take a recess before we go on to the solar experiment. I suggest that we have about ten minutes to stretch our legs.

(Short recess.)

DR. ROMAN: I think now that we see how the time is running, it is obvious that we will have to have a short session this afternoon. I think the arrangement will be that we will now go to a discussion of the Michigan solar experiment. We will then have a discussion of the engineering aspects of the orbiting observatory and a few brief remarks on the engineering aspects of the experiments themselves. We will try to break for lunch at quarter to one. I suggest that we reconvene about two o'clock. Then we will devote the next period of discussion to the management and administrative aspects of the program. We will finally end up after that with two movies, and one prepared by the Ames Research Center, and the other prepared by the Langley Research Center, on the work that has been done on guidance and stabilization problems for this project.

I will now turn the session over to Dr. Liller.

SOLAR EXPERIMENTS, UNIVERSITY
OF MICHIGAN, BY DR. W. LILLER.

DR. LILLER: I don't believe I need a blackboard for anything, so I shall stay right here.

First, telling you what we would like to do, our experiments are entirely on the sun and the solar atmosphere, because there will be certain simplification in the pointing system, presumably some of the payload space, and weight which will be needed for the other experiments will not be needed in ours. We will probably like to use this additional space for data storage capacity.

I have divided the brief description here that I am about to give you into six different areas. So let me just run down these.

First, what we want to do. We want to study various areas, small areas on the surface of the sun. These areas will have a maximum size probably of a square minute of arc. Remember now that the diameter of the sun is a little over 30 minutes of arc. These areas may contain nothing more than the quiet sun surface, and we may wish to note this for a study of the solar area from the center out to the edge of the limb of the sun to see how it varies to get information from the height distribution of these different types of radiation that we will look at.

We will also be looking at excited areas, solar flares particularly, sunspots, prominences or coronal regions which should be fairly bright in certain radiations. These areas would be selected in advance, perhaps as the satellite is going overhead, and the satellite would then be programmed to observe the particular area which has been chosen, and then the equipment will be started.

No. 2, the instruments that we will carry are quite different from the ones that you have been looking at so far this morning. Instead of having a large telescope with a small spectrograph attached to it, we will have a relatively large spectrograph with only a small collecting surface. The diameter of our collecting mirrors will be measured in just an inch or two for the most part except perhaps for the far UV and the x-ray regions. We intend to have three grating spectrometers. The first one will cover the range from 1500 to 3,000 angstroms with a photomultiplier scanning the spectrum as we rotate the grating. Secondly, a spectrometer covering the region from 500 to a little over 1500 angstroms to get some overlap again with a photomultiplier output, and the third spectrometer will cover the region from a little greater than 500 angstroms down to as far as we can go. We think this will be under a hundred angstroms down to maybe 75 angstroms, again with a photomultiplier output.

The fourth instrument of the four main ones that we intend to have will be a spectroheliograph or in this case called a spectroheliometer, which will be set on the Lyman alpha line of hydrogen at 1216 angstroms. An image of the sun will be produced in this radiation and will be monitored by a vidicon, either by command or by storage.

There will be posssibly additional small experiments, x-ray counters, for example, which do not take up much space or weight; possibly a second television receiver camera will be used to photograph the sun in other wavelengths, perhaps, than in the x-ray regions. Third, the operation of the system will be run something like this.

Each spectrometer will have two, possibly three, scan speeds. The fast scanning speed will give us a total scan of the particular wave length region which that spectrometer is intended to observe in a relatively short time, two minutes. The sun is bright. We can do this and we can get quite a bit of information. Two minutes is chosen to be roughly a tenth of a lifetime of a solar flare, probably the shortest lived event that we will observe on the sun. This scanning will run continuously as long as the sun is up above the horizon and fairly well out of the atmosphere of the earth. So this will be rapid scans back and forth during the orbiting time.

There will be a second speed, a very slow scan, one scan per orbit, that is, it will last of the order of 45 or 50 minutes, the length of time the satellite is in sunlight. It is one scan per revolution in this case.

All three spectrometers will have an arrangement such as this. A spectroheliometer will scan relatively slowly. It can be a number of seconds. This we have not looked into entirely yet. The total number of pictures which we will take per orbit depends on how much storage capacity we have. This I will come back to in just a moment.

Section 4 is on the information rates that we will be sending back and the storage we will need. For a fast scan we estimate that we will have for any one spectrometer several hundred bits per second, information bits per second, or a total of about a million bits for a complete orbit. This is with the fast scanning going all the time. The slow scan will not give quite as much information per revolution. It will be about two times ten to the fifth information bits. The spectroheliometer, the total number of bits depends on the resolution of course that we can get, but presumably it would be of the order of a million information bits per scan. We think in terms of a picture 200 by 200 picture elements with a dynamic range of perhaps a hundred or something of that sort. We would then have,we roughly estimate, a million information bits. As far as the storage is concerned, we would like to be able to store the order of ten to the seventh information bits, which is really quite a lot. It does not seem impossible, though if we use tape, although of course the moving wheels you have in a tape recorder should be avoided, if possible. So, core memory systems or storage tubes, that is something that we have to look into yet, and we intend to do this with some care. If we can get ten to the seventh storage bits, we should be able then to take a number of spectroheliograms during one orbit,of the order of 10 pictures perhaps. We would settle for less, naturally. We may have to settle for less.

The actual procedure: We picture an operator at one of the receiving-transmitting stations on earth He would have available the most recent spectroheliogram or, preferably, if the satellite were in sunlight at the time it went over the station, the operator should be able to command the satellite to send a picture of the sun in Lyman alpha as it appears at that instant. This would be done presumably in the time which is something less than a minute, and he would then have the several decisions to make.

First, what area of the sun to point at, whether it be a flare or a sunspot or just the surface of the quiet sun, or out in the corona somewhere, and we hope that he will be able to point to the nearest minute of arc. We envision a console where he can by step command have the pointing axis of the system move a certain number of minutes of arc to the east or west or north or south, and then the operator must make the decision whether to start slow scanning or fast scanning in operation or none at all, of course. He does not have to start it at that time. So he has basically these two decisions, where to point and how fast to make the observations. Once the pointing axis is located on the sun's disc, the guiding should be of the order of one second of arc. The granulation of the sun's surface, as the Princeton photographs from the high altitude balloons have shown, shows that there is a great variation in intensity across the sun's disc, and if we are looking at the order of a square minute of arc, we would want to hold it fixed on that square minute of arc to about one second of arc.

There would also be interrogation command, too, for the information to be sent back and recorded on the earth.

Finally, listing the different commands that we should have, we should have an east-west coordinate control. As I mentioned, a step command with perhaps 60 steps of one minute of arc. This would enable the observer to go out into the corona, go off the solar disc, and observe in the x-ray or far ultraviolet regions the corona lines. Secondly there will be the same arrangement north and south. Third, a fast or slow command. Fourth, a command to start a television scan immediately and transmit it back directly with no storage. This the operator would do when he is ready to make his choice. He would push this button, the scanning would start -- first, the transmitter would be turned on, and then the scanner would start after sufficient warmup time. The fifth command would be simple interrogation.

I forgot to mention in section 4 actually that the dynamic range we would like to be of the order of ten to the sixth, ten to the fifth would be satisfactory.

I think that is all.

DR. ROMAN: Are there any questions on the solar experiment?

MR. STEINMAYER (Bell Aircraft): Dynamic range of what?

DR. LILLER: Of the order of ten to the sixth, but ten to the fifth would be pretty good for our purposes. We would like to get a one per cent accuracy on line intensities. I might mention that the ultraviolet and x-ray spectrum of the sun below 1500 angstroms will be primarily bright emission lines, fairly sharp, fairly discrete, Above 1500 angstroms it would be a dark line spectrum primarily.

MR. TRIPLETT: You mentioned pointing the vehicle to one minute of arc from the ground. You mentioned also one tenth of a second of arc. Have you given any thought to what detector you would use to give guidance to one second of arc, how this is used automatically?

DR. LILLER: Some of the pointing controls that have been developed at the University of Colorado by Ball Brothers seem to be the type of thing that would be needed. Whether this can be pushed to one second of arc I cannot say.

MR. TRIPLETT: I was wondering, could you get it effectively locked onto particular areas or whether you would have to look at a star for reference, or some other means?

DR. LILLER: Two axes control would be quite easy where you just lock on the limit of the sun. The third axis would be a little more difficult where you would want to pick up the star. I suppose it would be all right, too, to set on a sun spot. The sun spot would not move appreciably during the time of observation. I think this would probably be sufficient actually.

DR. ROMAN: Would your observation time be short enough so that you could use stars or would the motion between the sun and stars be severe?

DR. LILLER: That is something I could calculate quickly. I think it would not be that large.

DR. ROMAN: A motion of a degree a day roughly?

DR. LILLER: Yes, this would be four minutes of arc an hour, something of that sort.

MR. KERPECHAR (Kearfott Co., Clifton, N. J.): I don't understand why you must point this accurately. Will you perhaps review this point briefly?

DR. LILLER: Your question is why do we want to point this accurately?

MR. KERPECHAR: Yes.

DR. LILLER: Well, many sun spots are of the size or even smaller. Solar flares, for example --

MR. KERPECHAR: The resolution on the sun then is the determining factor, not the resolution of the equipment.

DR. LILLER: That is right. Our resolution is generally something better than this.

DR. HELVEY (Radiation, Inc.): Is it envisaged that in this observatory there will be sensors incorporated for other than electric magnetic radiation?

DR. LILLER: We have not included this as one of the primary instruments. Certainly I listed additional equipment such as x-ray counters and so forth which could certainly include particle detectors, too. I think this would be very valuable.

DR. ROMAN: I think primarily we can say that this project is not designed for non-electromagnetic radiation. Any equipment of that nature which would be carried would be definitely of a secondary nature which would go along because there was room and because it could use the vehicle. But the vehicle would not be designed to carry it.

DR. HELVEY: I could see some very great advantages to have a co-incidence in certain operations of both.

DR. LILLER: Very true.

DR. ROMAN: Any other questions?

MR. GOOD: (RCA Astro-Electronic Products, Inc.) Would you care to develop any more information on your spectroheliograms? For example, how you intend to obtain the image of the sun, what resolution you will find on your TV tube and so on?

DR. LILLER: A very nice spectroheliogram has been photographed already from a high altitude rocket by Richard Tousey of NRL. His system is quite simple and we intend to use it. It is nothing more than two concave gratings, one of which forms a slitless spectrum of the sun, Lyman alpha being extremely bright and therefore relatively isolated as far as bright radiations are concerned. It stands out very nicely. You put a circular diaphragm over this slitless image, and then a second grating puts it back together again, enlarges and focuses onto the vidicon. The actual resolving power available here is limited simply by the, or primarily by the diameter of the gratings of which the resolving power should be of the order of a second of arc or so. The ultimate resolving power will be set, I am sure, by the vidicon, itself. So if we had 200 by 200 picture elements, and we filled the sun up in the screen, in the field of view, we would have something like a third of a minute of arc resolution.

MR. HUTTER (RCA Astro-Electronic Products, Inc.): The ten to the seventh bit storage you mentioned, is this strictly storage corresponding to the spectroheliometer image, rather than additional information from the spectrometers?

DR. LILLER: This storage was total storage, the spectroheliometer images plus the scanning information that we have.

MR. HUTTER: Do you expect to break this up as to how many bits would be in the spectroheliometer?

DR. LILLER: About 90 per cent.

MR. MEINEL (Kitt Peak National Observatory): Are vidicons sensitive to Lyman alpha or are any available?

DR. LILLER: No, as far as I know there is none available, but 1963 is still a little ways off.

Lithium fluoride is still fairly transparent. Perhaps a window can be made.

DR. ROMAN: Any other questions on the solar experiment? Are there any other questions on any of the experiments?

MR. BOROUGH (Boeing Airplane Company): Dr. Rogerson, the angular field view of the spectrometer, including the optical system, will have a five micron slit?

DR. ROGERSON: That is one degree. That will be for the fine TV.

MR. BOROUGH: The spatial angular resolution with five micron slit of four inches in diameter?

DR. ROMAN: You mean what does five mincrons at the focus correspond to angularly?

MR. BOROUGH: I thought it was about eight angular seconds.

DR. ROGERSON: I think it is on the order of a tenth of a second of arc.

MR. TRIPLETT: I think it is .08 of a second.

DR. LILLER: Ten to the second.

MR. STRAUSS (Aircraft Armaments, Inc.) I am thinking now of the structure which will support this imposing array of optics and the fact that the sun will rise and set on it twice every hour or so, and therefore introduce some thermal expansion. Which of these experiments can be designed in some nicely symmetrical way and which can not?

DR. LILLER: I might say that certainly optical designs have been devised whereby you have, say, a steel supporting member which expands outwards with increase in temperature and an aluminum pillow on which the mirror sits which will expand backwards and counteract the thermal effect. In theory anyway these thermal effects on the focusing can be avoided. They may cause changes in thermal noise of the photomultiplier.

MR. STRAUSS: There are, for example, off axis objective in many cases. I wonder whether the gentlemen who have these experiments have considered this symmetry problem.

DR. DAVIS: Our depth of focus is much larger than you usually think of for these instruments.

DR. ROGERSON: In our experiment we hope very much that one side of the satellite will be constantly oriented toward the sun, and there will be then a thermal barrier, double silver layer between that and the compartment containing the optics to minimize just this problem. That will give you one hot or at least warm region where you would want to put the electronics anyhow, and one very cold region which will be good for the photocells. By cold I mean something like a minus 100 centigrade.

MR. MEINEL (Kitt Peak National Observatory): There is a problem common to all of them that has not been discussed. How do you reacquire or what do you do about guiding when your objects are not visible to you?

DR. ROGERSON: That has been a great concern to us too. We had hoped to have an anti-stellarscope that looks 180 degrees from our primary instrument plus or minus a degree. We hope to be able to find a star brighter than the ninth magnitude that fulfills the requirement for any program star we are interested in. If the earth occults our primary instrument, then the anti star can take over the activeguiding. The accuracy need not be very great, enough so that we can keep it when it comes around.

DR. ROMAN: There are other possibilities for this. There are some problems involved with the anti-star. Some other possibilities that have been mentioned are that you could use a gyroscopic reference system for this length of time. In theory if you can keep your satellite well enough balanced, you don't have to have any active guiding.

MR. MEINEL: There is a tolerance regarding that, because that is one of the obvious solutions.

DR. ROMAN: I think the tolerance regarding it is that then it simply cannot get off so far that the guiding system cannot lock onto it again and start tracking. This depends entirely on the guidance system, that you have. I know this is sidestepping the answer, but I think this is one that you can't say unless you specifically define what you are using for guidance.

MR. KIERSTEAD (Goodyear Aircraft Corporation): If there are more than one experiment in a satellite are we to understand that only one will be run at a time?

DR. ROMAN: Yes. As I said, there probably will be more than one experiment in a satellite. They will operate on a time sharing basis. I do not foresee trying to run more than one experiment simultaneously.

Any questions on the experiments? If not, then, I think we can get to the engineering parts of the program. I will ask, after a brief interval to erect the screen, Mr. Triplett of the Ames Research Center to describe some of their thinking on the engineering and to bring up to date the engineering problems on the project.

ENGINEERING ASPECTS OF THE ORBITING ASTRONOMICAL OBSERVATORIES, NASA AMES RESEARCH CENTER, BY W. C. TRIPLETT.

MR. TRIPLETT: We have heard something from the experimenters of the type of optical equipment and instrumentation needed to do this job. I would like to make a few remarks on the engineering aspects of the problem.

First of all I might mention what our interest at the Ames Research Center is in this project. We have responsibility for doing research on the various engineering aspects of the astronomical satellites. We are also in a position to act as consultants both to Space Flight Development and also the industry and thirdly we will help in evaluation of proposals and also in the writing of specifications.

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First, this slide is to illustrate what some of the engineering areas are. They are of prime importance in this project. We have about five major areas here. This has been weighted more heavily in attitude control because I think this is one area that is unique to this particular project. Some of the requirements you have just listened to as far as pointing accuracy, the attitude control will be specialized. Some of the areas are common to other satellite systems. As far as attitude control goes, we look at various system concepts, that is, means of producing a control torque. We have listed here four possible systems, first reaction wheel control, motor driving the flywheel, which motor will produce an equal torque on the vehicle.

This also may be accomplished by a gyroscope in which the flywheel turns at constant speed, and a torque is produced by changing the spin axis of the gyro. These are two possible schemes involving momentum transfer.

A third system that looks promising both for fine and coarse control is the vapor jet system. This is a very low pressure gas jet system. It is very simple in concept.

The fourth system here is cold gas or high pressure jet system. This may be of some use in the initial stabilization of the vehicle.

Another integral part of the vehicle attitude control is error sensors. The type of error sensors used will probably dictate the type of control you can use. Here are three of them.

First is the television for acquisition and course guiding. This is in a sense an error sensor. The operator from the ground uses a visual picture as an indicator of the vehicle attitude.

Second we have the solar tracking. We also have star trackers or error detectors that make use of stars, that are being used in the experiments.

The third area that must be considered, in fact is one of the most serious areas I think in specifying attitude control, is the effect of external disturbances on the vehicle. We know well of torques due to the earth gradient, due to solar pressure. Those appear to be most significant. We also expect torques through the earth's magnetic field, perhaps due to the effect of the earth's atmosphere on the vehicle. These put very strict restraint on the system to be able to track to the accuracy desired. In that sense it means that the vehicle must be balanced in order to eliminate the earth's gravity torque. It must be earth symmetrical as precisely as possible. Consideration must be given to locating the center of pressure, solar center of pressure, coincident with the center of mass. I think those are the two outstanding effects.

The fourth concern in the attitude control system is the dissipation of angular momentum. If you use a momentum transfer system, such as A and B, then some means must be considered to get rid of the momentum that is finally stored on these controls -- there are a number of ways to do this. Either by use of a solar sail, or a sliding weight has been suggested, it means changing the inertial characteristic of the vehicle. It is possible to use a jet system, apply an impulse periodically. I think in regard to the experiments, the time this operation should take place is during the time, generally viewing stars, at sometime during the orbit the star will be occulted. This will be the logical time to remove momentum from the control wheels so that there won't be any undue disturbance during observation periods. This implies, then, that the control wheel should have enough capacity to last for one orbit without saturating.

We will come back to the attitude control a little bit later. We will go on to the other areas, engineering areas.

The second area is the power system. We have thought mainly in terms of a solar cell system and storage batteries. They represent a state of the art development. They are available. We have some experience with how they work. Solar cells appear to be very reliable and reasonably efficient as long as they can be kept cool. Storage batteries on the other hand, if we assume a vehicle lifetime of one year and charge-discharge cycle every orbit, the vehicle will be in darkness part of the time of every orbit. That means that the storage batteries must have a cycle lifetime of about 25,000 charge-discharge cycles.

Now, nickel cadmium storage batteries do have this potential. However, the charge cycle is shallow. In other words, the charging rate is not too high. However, experience has shown that a significant number of these might fail even below a thousand charging cycles. So there is some problem there on reliability of storage cells. It may mean in essence an over-design of the whole power system.

A third area is thermal balance. Here are some very unique problems, not only in regard to thermal gradient across the optical systems that were mentioned previously, the fact that some types of photocells, photo detectors, must be kept very cold, perhaps minus 100 degrees, or something in that order. Storage batteries, other types of electronic equipment, must be warm. Solar cells must be cool. Their efficiency deteriorates very rapidly with increase in temperature.

We have a number of thermal problems. Thermal problems are complicated; even though you have a vehicle that points toward the sun, it will be in darkness part of the time. Apparently the ratio of time in sunlight and darkness will change during the year, depending of course on the inclination of the orbit. At one time it may be 60 per cent of the daylight, at other times of the year 80 per cent daylight.

The fourth area here is communications. This includes radio command systems, both receivers, transmitters, data storage system, and also data transmission systems. Mr. Foster will have a few words to say on this subject so I will skip right over it.

The final area is in the layout of the vehicle. Here we are concerned with the final vehicle that may include more than one experiment. It is restricted in weight, it is restricted in dimension bythe size of the booster. It must include all the other factors we have considered. It may be required, for example, in regard to the solar cells in order to handle the thermal problem, it may be desirable to put the solar cells on paddles, rather than as part of the vehicle. In any event, all these considerations must be worked out in arriving at a final vehicle layout.

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These are some of the problems we have been trying to look at at Ames. So far our work has been mainly devoted to attitude control systems. We have been looking at various types of systems, various concepts. We have tried to evaluate them on this basis.

First of all, we must have a dynamic performance, even a steady state performance that is consistent with the requirements of the experiment. If the system can meet these requirements, then we would like to evaluate and compare different systems on the basis of these other factors. That is power, weight, reliability, and availability. We may lean towards things that are available today rather than something that is in the state of development and may or may not be ready by 1963

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Now, in order to really evaluate control systems realistically we have to set down some requirements. I don't look at these really as requirements, but more as ground rules under which we must operate. We can set down some of these, it gives us a common basis for comparing different types of systems, different concepts. So we look at the control problem as really in three phases.

The first phase is the initial stabilization. We are considering an initial tumbling rate as high as one degree per second, which is quite high. We are assuming that this initial stablization system -- it may be a jet system using a gyro reference -- should be able to stabilize the vehicle rates to within .002 of a degree per second. This seems within reason on the jet or gyro systems. We have considered with the same operation that the vehicle will be oriented in space so that the solar cells will be pointed toward the sun within approximately one degree. In this position, then, the vehicle would be essentially stabilized in space where its attitude then could be determined from a remote control television picture, and then from this point the acquisition of target star could be achieved.

Now, as far as acquiring a star, one of the requirements specified has been a maximum slewing rate of 180 degrees in five minutes. The ground observer by remote control can acquire any star visible within the time that the satellite takes for one pass over the ground station. This is really quite an excessive requirement, excessive from the standpoint of importance required. We also were thinking in terms of remote control pointing the vehicle to the plus or minus 15 minutes of arc. On some experiments, particularly the solar experiment, they would like to have this done to one minute of arc, but this was, I say, one set of ground rules.

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The third phase of control would be the automatic control. We are looking both at coarse control and fine control. Here is a coarse control that would really be a continuation of television acquisition. After the error got down below a certain size, then an automatic mode would be switched on in order to bring the error down to the limits of the coarse control system. So here we were thinking in terms of a detector that would sense initial errors as large as 30 minutes of arc. This coarse system would provide a final vehicle pointing of one minute of arc.

You will recall that in some experiments this is perhaps all the accuracy that is required so that a coarse control system could do the whole job. Again during this phase of the operation, we would still have this residual .02 of a degree per second vehicle rate that will be carried through. We would have external torques of nearly one hundred dyne centimeters. This would be what we think is an extreme figure, but it is a sort of torque that could be expected with center of gravity -- I should say moments of inertia of the vehicle equal to within one half of one per cent, and also with center of solar pressure just a very few inches from the center of gravity of the vehicle. You could get steady torques that would act in one direction for longer periods of time on this order of magnitude. This is the total angular momentum of the vehicle if it were slewing at the rate of 180 degrees in five minutes. I don't think that figure is really significant here.

The final phase of the control would be the automatic fine control. We are considering an initial pointing area of two minutes of arc. There would be some overlap in fine and coarse control. We are considering ultimate pointing accuracy of one tenth of a second of arc. Again the vehicle could have initial rates as high as .002 degrees per second. It will be subject to external torques of 100 dyne centimeters. One of the requirements -- I think this is the more serious requirement as far as pointing accuracy, even more serious than one tenth of a second of arc -- is to hold down the steady state or the drift rates due to this 100 dyne centimeter torque. Depending on the nature of the system, if it has an effective integration when the torque is acting on it, there will be a steady state pointing error. To keep the limit within this tenth of a second of arc we have to specify that this steady state due to the 100 dyne centimeter torque will not exceed five seconds.

We have also specified that the saturation time, that is, if a reaction wheel system is used, should be 100 minutes.

There is one other interesting thing that was mentioned earlier, the aberration of light. That also puts a stringent requirement on the system. In view of large disturbing torques that requirement is of secondary importance. If these torques could be reduced, and they may well have to be reduced in order to develop a system that can come anywhere near meeting these requirements, and then it turns out that the aberration of light will be one controlling factor to be able to handle a line of sight rate of some .3 of a second of arc per minute, that may be one of the most serious constraints.

(Slide)

I just want to mention briefly an example or two of the types of systems we have been looking at at Ames Laboratory.

First, these two are both reaction wheel systems. The first, to go through the diagram, we have an error sensor which detects the difference between the true line of sight and the target star, and the actual pointing direction of the vehicle. This may be modified by some passive network. The signal drives the motor wheel, producing a torque, subjects it to external torques, the torque is integrated to produce a integrated rate, integrated again to get vehicle attitude.

We have also considered a tachometer feedback possibly shaped by a network. This type of system does promise reasonable dynamic response, depending on the types of networks you have here. We are in the process of examining various types of networks in order to provide sufficient damping and to minimize the steady state.

The second system we might consider is very similar. Except for the tachometer feedback, we do have an integrating gyro. This from the standpoint of complexity might not be so desirable. It is an additional moving part but if you can consider a perfect rate gyro, then this offers an ideal type system because it gives you all the performance you desire. So it is what we might consider a standard system that we can use as a basis for comparing other systems.

Now, this type of system does have one additional advantage. When the target stars are occulted the loop is broken right here, the inner loop will operate and the vehicle will remain phase stabilized during this period of time. A system of this type can hold a vehicle to within a minute of arc for as much as 30 or 40 minutes. So there is one additional advantage you get in turn for additional compexity. When you consider this type of system, it may be possible just to remove the motor altogether to use the gyro, use the gyro in two modes of operation; use the gyro to provide torque for the vehicle and also use a gyro as a stabilized space reference. This is another area worth looking into.

One more type of control system we looked at a little bit is our vapor jet system. The nice thing about these systems is their complete total simplicity. There is no moving part other than the valve. We have the same sort of error sensor. We modify the error signal and put it through a logic network and the signal out of the network operates the valve. That is all there is to the system.

Here we have the same torque, external disturbances acting on the vehicle. This is essentially an on-off type system. It uses just one thrust level. It has two modes of operation. For large errors, we have the continuous mode. We have a steady thrust. Here we have a switching object. Here is the plot of the error versus error rate. We sense both of these quantities. This is a switching logic network that follows this form. Whenever the vehicle is out on this side, you get a steady thrust until it crosses this line. Then the thrust is reversed. It is essentially a bang-bang type of system. When the error rate gets down to within certain limits then the mode of the system switches, we have a pulse mode where a standard size pulse is triggered. In this case we set limits to one eighth of a second. Whenever the error reaches that value, you trigger the pulse. You end up in a steady state with a sort of limited cycle operation. For example, we have considered the period something on the order of 50 seconds. The pulse would actually fire every 25 seconds. If external torques are applied to the vehicle -- this is no external disturbance -- you get the same sort of pattern except it would always tend to go back to one side. Fuel economy for this type of system, we considered water vapor, specific impulse of about 40 and still for each channel this type of operation involves about two pounds of fuel per year. It is quite economical in fuel.

There are two problems associated with this type of system. First of all, any use of fuel will change the balance, inertial symmetry of the vehicle, and secondly what may be more severe is what will happen to the water vapor that is exhausted. Will it go in the form of a cloud around the vehicle? If so, it will destroy the experiment you are trying to accomplish. Until that question is answered we certainly could not consider using this type of system but it is interesting. It will have many applications.

(Slide)

I have one more slide to illustrate the pieces of experimental equipment we have been using at Ames Research

Laboratory to evaluate systems. We have developed a missile simulator here in which we have a large platform which simulates the missile, the satellite. We have this platform floating on an air bearing. You can see it on the middle of the picture. There is a supply of air which actually floats the ball, the whole platform is floated on air. It takes about 30 pounds of air pressure through an orifice, in the bottom of this disc to float the whole works. The table weighs about 200 pounds. So we have virtually a friction free table.

We have here a little telescope with an error detector and photodetectors on here. So we have a two axis control system.

Out of the view of the picture there is an artificial light source. So we can simulate the operation of an entire control system. Now, what we had at the time the picture was taken was a reaction wheel system. We do have one reaction wheel motor here that controls the vehicle in pitch. We have another reaction wheel here that controls -- we have enough pendulum effect on this platform so that there is no control on the third axis. There is some feeling for the type of control system, the stability problems we encounter in using actual hardware. We also get some feeling on the type of response we get from various error detectors. We are really just getting under way in this area right now.

I think that is everything I had. I know that John

Foster would like to say a few words about the communications area. So I will let him take over from here.

PRESENTATION BY JOHN FOSTER

MR. FOSTER: I really don't have too much to say on the communications. We have only looked into it to a large extent in association with the control system work. However, I will make a few observations here of what we have at least thought about.

The data system really will have to cover four different categories of data. First are the ones associated with the vehicle performance and command. This will be the state, for example, of the inertia wheels, whether it is saturation or not, various commands that go up to operate the optics and returning data that indicate that these commands have been performed, this type of data which will essentially be a rather narrow band type of telemetering.

Then there is a second type which is the acquisition TV in various of the experiments or on the platform itself there will be a TV system that looks at either a 10 or 15 degree field, and then finally in some cases down to one degree field for acquisition. This picture will have to be sent down and displayed properly to the operator.

The dynamic range of the light on this particular TV will not be as great as some of the TV required for the experimenters. Therefore, the band width requirement on this aspect of the TV problem may not be too severe.

The third thing, our type of data will be that required by a majority of the experiments noted today which are readings from photo tubes on the spectrometer type of experiments. In general this can be handled by a rather narrow band telemetering because it is an accumulated count deal, and the scan rates in general are quite long, so that the rate of accumulation of data is not high, and you have sufficient time to send it down by a rather narrow band width telemeter.

The last category is the experiments which have a TV scan type of data in their accumulation, such as that explained by Dr. Davis. I will go into it a little bit, what we have done about it. Let me say the narrow band data from the platform and from the experiment I don't think should present any real problem, so I won't dwell on that. The acquisition TV can be relatively lower accuracy than the data, probably should be reasonably easy to get within an available band, within limits of the available stations. So this leads to the last category which appears to be somewhat the tougher and with due apologies to Mr. Davis, whom I have not had a chance to talk with here, I would like to make some statement on which he can argue with me or not, and they may be incorrect so far as that goes. But in looking at the requirements of the TV scan associated with that type of program, that was where you made the whole sky sort of essentially a two degree sort of segments.

One of the bugaboos here is that the large dynamic light range that would be required for the experiment plus a reasonably high accuracy, a tenth to a magnitude star, which in a preliminary aspect was quoted at 10 per cent accuracy, this was based really on a tenth magnitude star accuracy, this ten per cent accuracy data. The thing is if you look at a 500 by 500 line frame, and you want to scan and send this data down in one second, you can come to a first low band width of 250 KC. This is simply multiplying the lines by the resolution across the line, but our feeling is that really you can't with this dynamic light range and accuracy requirement get by with that narrow band width.

Then you look at various other schemes, you could code each of these, each and every one of these possible 500 by 500 points. There you arrive at something around two megacycles band width. Well, we won't have a capability for that. So some place in between here will probably be the true case.

One other thing I would like to throw into this, possibly for consideration of the Smithsonian experimenters, is that you might look at this and say, well, there may be only 50 stars maximum in any one of these frames, and you say well, why worry about all the 250,000 picture elements? Why not just worry about the ones that have information on them? If you detect these, break them down into a binary code and store them with each individual position of course as the thing is scanned. From that aspect it turns out you can get down very low band width; it turns out to less than 3 KC for one second of scan of frame range.

A little problem comes up. There will be concern with luminous areas which are not contained in these 50 star points. So we thought possibly a method might be, a hybrid method might be evolved where you were scanning and storing the information from these 50 stars, the luminosity areas may be of a lower frequency enough that they can be handled with a rather narrow band width. At the end of this scan then you send out the ooded information of the 50 point sources, the stars, and maybe get by with a reasonable band width but I believe there will either have to be a method of this nature or possibly a longer scan rate to really keep it within the band width requirements. This is something that we have not really looked too far into except I think it will be somewhat of a problem. That is really all I have to say, unless there are any questions on that aspect.

MR. CHENG (Hughes Aircraft): Is there any reason why this must be transmitted in one second, other than the fact that your experiment will take longer?

MR. DAVIS: There are four television tubes. The one second was arrived at by using half of the motion time for the transmission of the signal. There is no reason why it could not be prolonged.

MR. CHENG: In other words, if this proves to be one of the nastiest problems of the whole experiment that you are trying to do, there is a possibility that you might compromise a bit on this and say well, we will reduce the band width requirement by going perhaps to two seconds transmission?

MR. DAVIS: Two seconds would not bother us. More than two seconds would actually start slowing down the experiment. We would start looking at coding methods as a possibility.

Dr. Dursey, how long do you think we could afford to stretch out the scan?

DR. DURSEY: Excuse me. I would like to tell you in your computation about the 150 kilocycles, actually if you consider just an analogue transmission, go to one half, 125, because if you have a 250,000 picture element on your picture and you consider the maximum presently involved in the situation, you have a complete aperture element followed by a complete black one. You actually have 125 kilocycles per second.

MR. FOSTER: I must admit that it is rather difficult to come to a band width requirement. We find we are not alone in not being able to come to exact figures in this. When you bring in the factor of your required accuracy in the dynamic --

DR. DURSEY: Absolutely. You must shift from an analogue type of transmission to a coded type. So the consideration of band width is completely changing. You are perfectly right. What I imagine Mr. Davis was pointing out was that with a normal TV system it is meeting with some certain number of letters like a normal TV system is doing, you may go down to 125 kilocycles per second band width in one second.

MR. FOSTER: I am not quite sure that I --

DR. DURSEY: You take, for instance, a quite usual television system with 30 frames per second, that is corresponding to four megacycles per second. How is the band width computed, just taking a certain number of picture elements, for instance, 500 lines horizontal resolution and 500 in vertical resolution. You have 250,000 picture elements and actually if you want to scan 30 times per second, you reach 50 times 125, so 4.5 megacycles per second. That is no consideration here about accuracy in the determination of the levels. That is the way I imagine the Smithsonian has followed. If now we have for every picture element to consider carefully a certain number of letters we have to visualize our picture transmission, and so we may go to a much higher, much wider band width.

MR. FOSTER: This is the thing I was bringing up. It is something that will have to be considered. I think in the early specifications, things of 150 KC and 250 were pointed out as probably covering the experiment. I think this is not true. It will have to be looked into more thoroughly.

DR. DURSEY: Who designs the experiment to fit into whatever band width you give us?

MR. FOSTER: Are there any other questions?

MR. COLLINS (Page Communications): Considering the aspects of solar pressure and/moment of inertia and all these things mentioned previously, is it possible to consider the use of six foot parabolic antennas on this vehicle?

DR. ROMAN: On this vehicle?

MR. COLLINS: Yes.

DR. ROMAN: I think this is for someone else other than me to answer. Do you have any answer on that?

MR. TRIPLETT: I think antennas is one thing we have not mentioned. I think that can be a serious problem from what I gather. The type of antennas you need for transmission still has the direction in which they have to be moved.

MR. FOSTER: Actually both the requirements of the center of radiation pressure and the CG, as you probably figure, will be very stringent if you have to move any external--

MR. TRIPLETT: You agree there is a problem, but you are not sure of the answer?

MR. FOSTER: That is about it.

DR. ROMAN: Any other questions?

MR. BENEDIKT (Norair, Northrop Corporation): (Remarks concerning sputtering inaudible.)

MR. McDONALD: When you quoted ten watts for power for transmission, Dr. Davis, what type of antenna did you have in mind?

MR. DAVIS: That was a sixty foot paraboloid on the ground, and isotropic radiator for the satellite.

DR. ROMAN: I would like to thank you for your remarks on sputtering. We have not gone into the environmental effects at all here today largely because these are being looked at elsewhere in the space problem and I felt that it was not for us in astronomy to tackle them as well. However, you are right that sputtering is something that will have to be investigated.

MR. CHATKOFF (Minneapolis-Honeywell): Since your driving torque is the function of the orbit, is there any method of keeping the orbit eccentricity down?

MR. TRIPLETT: The measurements we have made assume a circular orbit. The only way to reduce these torques, the main thing is to make the vehicle symmetrical. There is a problem. We have thought in terms of symmetrical to one half of one per cent. It is questionable whether you can even measure moments of inertia that accurately. That is the problem, to build this vehicle as symmetrically as possible. The other important one is the solar pressure.

MR. CHATKOFF: I was thinking of the eccentricity of the driving function in the unsymmetric inertia, the fact that your angle of gradient rotated.

MR. TRIPLETT: That would be an additional source of trouble. You would have to minimize the total effect of all the sources, at least minimize it down to the point there are a number of things that will be relatively uncertain.

MR. CHATKOFF: You have made no attempt to make the orbit perfectly circular?

DR. ROMAN: What do you mean by perfectly circular?

MR. CHATKOFF: By having a correction device on board to take out any eccentricity.

DR. ROMAN: I think we will probably depend on the vehicle that gets it up there to get it in as good an orbit as we can.

QUESTION: This type of orbit would be elliptical and not circular so it does not lead to saturation.

MR. TRIPLETT: That is right. It is the circular torques that will cause the trouble.

DR. ROMAN: Any other questions on either of these?

MR. CHATKOFF: Mr. Triplett showed a picture on a screen of a platform. I believe I got the impression that this was an experiment for stabilizing the vehicles by using your vapor jet and you had a sensor. Will you describe the sensor?

MR. TRIPLETT: The sensor is essentially a pyramid type. It is a four sided pyramid. So the light impinges on this pyramid. We get a signal. We have four photocells, one looking at each side. Then we sum the outlets of these two cells. As the light moves over to one side, we get more light in one cell than the other.

MR. CHATKOFF: You imagine you may utilize an optical sensor for control in orbit where you might be subjected to drift?

MR. TRIPLETT: When you get to the actual vehicle you may want to use a single cell, sharper arrangement. You may not want -- the sum of the signals of the two cells, yes.

MR. MITCHELL (Boeing Airplane Company): In this simulator and in your other control calculations have you included in your control logic the inertial cross couplings or have you done this on the basis of isolated action?

MR. TRIPLETT: We have looked on an analogue computer, at the effects of cross coupling due to gyroscopic effects, and also due to the product of inertia effects. They appear to be of secondary importance, at least to the extent where we think you can design a system based on single axis analysis.

MR. MITCHELL: Even systems to the accuracies we are asking for now?

MR. TRIPLETT: Yes, I think so, because the rates are awfully small, very small. I think you can specify the parameters.

DR. ROMAN: Is there disagreement here?

MR. CHENG (Hughes Aircraft): According to the preliminary requirements you mentioned as far as possible the present minitrack facilities are to be utilized for this experiment. Now we have looked into some of the gains from the antennas. It does not look like it is up to the value that will be needed for a broad band reception of data of the type Mr. Davis wants to have. We just wondered whether any concentrated changes are going to be made for the minitrack antennas and possibly receivers.

DR. ROMAN: I don't think it is in the budget at the moment. This other matter will be gone into in I hope rather appreciable detail before much longer. We will be able to I hope come up with an answer as to whether we can use a modified minitrack or whether we will have to go to something else.

MR. KAMM (Convair Aeronautics): You said you will not be using polar orbits because of the Van Allen belt. Is there any decision on what orbit you will be using?

DR. ROMAN: In our preliminary specifications we started 500 mile circular and inclination 30 to 35 degrees. I think we could drop down to 450 miles. We don't want to drop much lower because of air drag. We don't want to go much higher because of the Van Allen radiation.

QUESTION: In what manner does it affect you?

DR. ROMAN: It gives you noise in your photocells which are the same as dark current noise, for one thing.

QUESTION: Doesn't it require relatively few grams of shielding?

DR. ROMAN: Yes, but the shielding will shield the light.

DR. KUPPERIAN: That depends on what signal to noise ratio you want. It requires quite a few grams to completely get rid of it. On a single photon you start getting x-ray. You have to shield then.

QUESTION: The absorption cross section in the cesium film is low enough.

DR. KUPPERIAN: It could reduce it to some level. The question is what is the level. At the moment we are getting new surprises from the Van Allen belt as the data come in. Sometimes it gets lower, high effects appear at a low altitude. I don't think we are going to be able to avoid it anyway, even if we stay below 500 miles.

DR. ROMAN: I think we had better break for lunch and reconvene at two. If there is an interest in continuing the discussion of this aspect of the problem we can, and then we will get into the administrative side of it, the management side of it. Also, at the request of a number of people in the audience, I have written in order on the board the names of the speakers this morning.

(Thereupon at 12:45 p,m., a recess was taken until 2:00 p.m., the same day.)

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AFTERNOON SESSION 2:15 P.M.

DR. SCHILLING: Good afternoon. As one of you remarked to me a few minutes ago, all the paying guests were here at two o'clock. Our panel were taken to a luncheon meeting, and I am waiting, myself, for an explanation. I will turn it over to Dr. Roman.

DR. ROMAN: No explanation except we spent too long talking before we ordered.

I should like in starting the program this afternoon to take up where we left off this morning, and find out if there are any more technical questions on any aspect of the projects, any of the experiments or on the stabilization or communication.

MR. MEINEL (Kitt Peak National Observatory): I would like to ask a question of Mr. Triplett. He mentioned the cross coupling was a minor term. I wonder if that is really so. Suppose you have three orthogonal coordinates and only one inertial wheel as angular momentum, you slew that package 90°. The momentum is 90 degrees out of phase. You have to take all the momentum and feed it in here to keep your package stable.

MR. TRIPLETT: I was speaking from the standpoint of fine control. The only time you get any serious cross coupling is if you allow fairly large rates about the optical axis. You see, well, maybe the only thing controlling that is the solar orientation, controlling motion about the axis. As long as you can keep those rates low, then cross coupling, at least for fine control, is rather negligible.

MR. MEINEL: There is this problem when you move from one object to the next.

MR. TRIPLETT: Yes, slewing control. You must consider that. It might be advisable to slew one control at a time. You can get into some messy cross couplings.

MR. SAVEDOFF (University of Rochester): You mention plus or minus one degree on the solar batteries. Doesn't that limit the area the telescope can look at rather significantly?

MR. TRIPLETT: I think that was for initial stabilization. You would like to get the vehicle, one side pointed toward the sun or the side that has the solar cells pointed toward the sun to activate the power station. I think any experiment would allow more tolerance in one direction, perhaps up to 45 degrees, during observations where you can get 70 per cent effectiveness out of the solar area. Otherwise, you would have to orient the cells with the structure.

DR. ROMAN: Any other questions?

MR. BAXTER (Ryan Aeronautical Company): Have you considered the solar concentrating type of APU source as opposed to the cell type?

MR. TRIPLETT: To my knowledge we have not. I am not familiar with that type.

MR. BAXTER: I was wondering if there was any objection to this type or would it not be considered such a good idea?

MR. TRIPLETT: I would say there is no objection to any type at this time, anything that appears feasible.

DR. ROMAN: I think the biggest question is to increase the problem of radiation pressure. However, if that can be solved, I don't think that would be any objection.

MR. BAXTER: This would be the main consideration whether or not there would be any unbalance due to radiation pressure.

DR. ROMAN: That and of course reliability, and ability to supply what you want.

MR. MITCHELL (Boeing Airplane Company): Mr. Triplett, have you considered in that simulator of yours the viscose drag of the air bearings? Isn't this significant in the control?

MR. TRIPLETT: There is some drag. It is awfully small. I think with this type of equipment a more serious problem is what we call turbine torque. Any viscosity of the air around this ball -- the thing wants to rotate. This can be minimized by very close machining, very carefully polishing of both the ball and the seat it fits in. Still you have those to contend with. In fact, they can be quite large as compared to the 100 dyne centimeters we are talking of here.

MR. GILLESPIE (LRC): On these turbine torques we have found that we could minimize this effect by tilting the base to the point where the ball would no longer rotate. I think this would appreciably reduce the torque in the ground simulation test.

DR. ROMAN: Any other questions? In that case I want to make a few remarks in regard to the thing we have been talking about.

One item you might have noticed is that each of these experiments has been approached from a somewhat different angle. Some of them have come up with specifications which are not in complete agreement with the preliminary specifications and some of the specifications are slightly contradictory to one another. On that my answer is that there is not any really right or wrong about these at the moment. We are all in the stage of planning and trying to find the best way of attacking the problem. The preliminary specifications were written to try to give an idea to you people of the sort of things we had in mind. Some of the requirements are firm. In other cases, though, we could afford to back down a little if it turned out that what we were asking for was impossible, or if not impossible, at least impractical. There are still other cases where we would be willing to trade one type of behavior for another. To take a very simple example, in the television finder system, the preliminary specifications indicated we wanted a 15 degree field, we wanted to see six and a half magnitude stars. As it turned out, if the 15 degree field was too large on which you could get a television camera that would go to fainter objects, I think we could live with a two to three degree field if we went to perhaps ninth magnitude stars. I give this as an example of the type of trade off we would be willing to consider.

In the case of the contradictory approaches of the various experiments this is something that will have to be ironed out, I think all of us are still at the stage of doing planning and trying to find the best way. Eventually as far as possible we will have to find one common method of attack. We have approached this problem with the idea we would not design a new vehicle for every astronomical experiment, and I think we are still working on that plan.

In addition, as I mentioned earlier during the question period, we are not planning to devote an entire vehide to a single experiment. This means that the experiment will have to be made compatable with a common vehicle.

I have one other thing I would like to mention at this point. That is the specifications, the preliminary requirements for the optical detectors which most of you picked up at the door this morning. You may not have had time to look at them. I think a word on those is in order.

We have said that the optical detectors will be the province of the experimenters. I am not going back on that statement with the exception of the television finder system of course. However, a number of you have been asking me what type of detectors will be used in this problem. Since the light levels and wave length regions are different from those which we think about when we talk about electronic devices, Mr. Dunkelman, a staff scientist for optics and detectors, has prepared the preliminary requirement to give you people some idea of the types of systems which the experimenters and we will be interested in.

I think with those comments, I will turn the rest of the afternoon session over to Dr. Schilling.

DR. SCHILLING: Thank you, Dr. Roman. I think we are getting now into the presentation where we thought you may have questions of a general nature, including controversial questions and perhaps some questions of what comes next.

Let me mention again that today we are talking about a second generation of experiments. I don't think I have to define second generation experiments. If you want some definition, something like we have more problems than are solvable at the moment.

Let me start this off with amplifying one of the remarks that Dr. Roman made in her introductory speech. Dr. Roman mentioned that the preliminary specifications which were prepared by Ames Research Center and sent out a while ago talked about the Vega vehicle. The Vega vehicle is a two stage vehicle to place the satellite into orbit. As Dr. Roman pointed out, no decision has been made as to whether the specific stabilization platform vehicles for astronomical labs will utilize the Vega or the Agena B. The capabilities as far as satellite orbits are concerned are just about the same, so it might well be that we will use Atlas Agena B.

According to this preliminary quick look, which we have had, this would not change materially the specifications which are in your hands and anything which you heard today. It might affect, if anything, the time, when we talk about the first launching. You heard throughout today 1963. We might as well say the end of 1962. We don't know. I think I don't have to explain where our budget stands. You all know that as well as we do.

QUESTION: When will you buy the astronomical platform?

DR. SCHILLING: As soon as we have some money.

QUESTION Do you think it might be in 1963, '62 or '61?

DR. SCHILLING: As Dr. Roman pointed out, the planning for test projects is not just budget alone. We have other problems which could not be solved much faster even with cash. So I think our planning will go ahead for 1963 without specifying at this time whether it will be launching very early in 1963 or very late. This may change both ways. I can not see that it will come much earlier than the middle or late 1962 if we are optimistic. Could I have some comments from the panel, if anything earlier than 1962 would be realistic?

I think that we have a general shaking of heads.

QUESTION: Launching in 1962 or procurement in 1962?

DR. SCHILLING: We are talking according to the specifications about launching in 1963. Perhaps to amplify this a little bit, at this moment we have funded some preliminary development contracts on a mostly study basis. Moneys that have been funded on the observation astronomy program have been in the general area.

DR. ROMAN: There is a little bit going on in that area. There are also several other contractors, some in optic.

DR. SCHILLING: Forgetting the procurement of the booster system, Vega or Atlas Agena B booster systems, when we are talking about procurement, we are talking about the next fiscal year as the first possibility for large scale development and eventual procurement. I want to throw this open to general questions on this aspect. Are there any other questions?

MR. BAXTER (Ryan Aeronautical Company): Is there any particular process we can go through in order to interest you people in specific ideas? Whom would we see, for example?

DR. ROMAN: I think I have been talking to a number of you people individually, and very many of these same questions have been coming up. When can we do the funding, and what. I think I have told most of you the same story that we cannot do the major funding this fiscal year. However, we do have some money for small study contracts. These I emphasize will not cover the whole system. They are not intended to be that large. I do have a list here of areas on which I think we might consider study contracts. I will read this list quickly.

The course orientation and slewing system, that is, how you get from one region of the sky to another and settle down there.

The fine stablization system. I think this is one we have all gone through enough. There is no particular difficulty about it.

Means of balancing the satellite in orbit. It has been brought up several times that the satellite is going to have to be accurately balanced. There have been questions raised as to whether you can measure balance ~~into~~ one part in ten to the third while the vehicle is on the ground in a strong gravitational field. I think we might be interested in looking into ways in which we don't have to do it thoroughly from the ground.

The effect of magnetic and gravitational fields. This has come up several times today. It may be that there has been enough work done on that already so that no further work is needed.

An acquisition television system, a finder telescope.

A backup system for this. I feel we would be rather unhappy to pin the whole experiment on a television system. We would like some other way of finding out where the telescope is pointing or directing it to another part of the sky in the absence of a working TV system.

The data storage system has come up. In particular there I am thinking about the storage for experiments such as the Michigan experiment which Dr. Liller described,, and the experiment which Mr. Davis described. I have already covered the detector field. Eventually we hope to prepare some preliminary specifications in the area of communications telemetry and data handling similar to those which we have distributed in the other two fields. These have not yet been prepared. I imagine it will be a month or two before they are. These are areas and the types of areas in which we think we would consider study contracts.

As to when we will request these, I think you will be hearing from our procurement department in this matter. This would go through them, and all of you who are here today, who have signed cards and let us know that you are interested, will receive any information on that when it is distributed.

For the sake of the completeness, I might mention two areas which have been brought up several times which I do not think we wish to pursue very strongly at this point. One of these I have mentioned already, the effect of space environment on materials and components. The other is/the power supply. The reason is not that I do not feel that these areas are important. They obviously are highly critical for the success of this project. However, they are also critical for the success of many other projects, and there are rather large programs already under way investigating the possibilities and the problems in these fields.

DR. SCHILLING: Thank you. Are there any other questions on this subject area?

MR. REICHENBACH (Kearfott Co., Washington, D. C.): Can you outline how NASA intends to organize the system and subsystem? You have one prime contractor that will manage the subsystems?

DR. SCHILLING: I will again ask Dr. Roman to answer, but along with some introductory remarks. Some of you may be familiar with our present practices which go both ways. Normally NASA retains top management control for all its projects. Prime contracts have been let, like, for example, in our Able Project where the prime contract is let with the missile division of the Air Force with the Space Technology Laboratories. That is one instance where the Space Technology Lab is engaged in the contract.

We have another way. It goes to the Space Flight Center where we are building up, taking scientific capabilities as well as supervisor capability, we will assign various system management responsibilities.

As to the specific project here, no final decision has been made, and will not be made for a while. Dr. Roman may have some specific comments.

DR. ROMAN: I don't think I have very much to add to what already has been stated. The decision has not been made as to whether we will do the major system management in-house or in industry. However, I think that a large share of the contracting, a large share of the work other than system management will certainly go out to industry.

DR. SCHILLING: At some later time we may invite complete overall system management, but as has been pointed out, this has not been decided. System management control will be exercised by our staff at one of our centers. This may be Ames Research Center, Langley, maybe in cooperation, with a small task group and a project manager.

MR. STEINMAYER (Bell Aircraft): Can we pursue that a little further? When you get to putting your hardware together, am I right in thinking that you people will probably subcontract the actual joining of the systems into one package and check out to some group?

DR. SCHILLING: That would appear very likely. In other words, we have various possibilities. To answer more precisely, normally in experiments we can talk about field packaging, where it takes an upper stage and put a payload on top. This payload comes from all kinds of places, various universities contribute experiments and so on. We usually have this payload packaging done. At this point, the Army Ballistic Missile Agency has done quite a bit with us in the Juno Program, and is doing it now. Again in our Able Project, it was done by STL. It is very likely that some of these things can be done by the Space Center. When we get to the second generation experiments like this one here, when we talk about experiments, these are not small projects any more, but we have to talk about payload systems.

In addition to the instrument which our own Center will prepare actual assembly of the complete vehicle and launch may be by the Army, Air Force or Navy or through mutual cooperation. It is very likely we will look for a contractor for everything which is above the basic booster system, and ask for complete assembly. However, we will not ask this agency to do the subcontracting for scientific experiments.

This is a long answer. Are you thoroughly confused?

DR. ROMAN: One thing we have been thinking about in this is that we might like to reserve the right to specify subcontractors in certain areas. This would of course apply to the experiments and you have mentioned it might apply to the telemetry.

MR. STEINMAYER: Pursuing that one step further, assuming you would have some contact do that for you, would you care to make any non-definitive comment about what that contractor participation might be in the space program?

DR. SCHILLING: Instead of answering in the future let me go back and give an example. If you remember the Pioneer flights , something similar was in effect, namely, where ABMA did part of the launching, as I remember, and some of you have been involved, I think the responsibility after the vehicle was up a certain altitude. Such an arrangement might be possible. This of course depends on the capability. Also a mutual arrangement. There may be a possibility of two or three contractors joining together. Are there any other comments or questions along this line?

MR. TURNER (Republic Aviation Corporation): What do you visualize would be the ultimate funding to get seven vehicles into space in 1963?

DR. SCHILLING: Orbitting as astronomical observatory projects?

MR. TURNER: Yes.

DR. SCHILLING: I think a comment was made that our panel was probably thinking of about one launching a year, is that correct.

DR. ROMAN: I don't think we actually stated that. That is what we would like to see.

DR. SCHILLING: As long as it is not one every month, I am happy, but to give you a figure would be really just artificial. Of course, we have gone through budgeting, we have gone through detailed budgeting, but with so many problems unsolved, so much depends on when and how expensive are the solutions. If I throw out something like $25 million that is in the right ball park not counting the boosters. Would you agree with this? Dr. Kupperian has been in the budgeting for a long time. Would you care to stick your neck out?

DR. KUPPERIAN: If you don't have any troubles, you probably could.

DR. ROMAN: I think the real answer to this question is that the person who asks it and the others in the audience could probably do a better job of telling us than we can of telling them.

DR. SCHILLING: Any more questions along this general line of managerial arrangements, contractual arrangements?

MR. BAXTER: I don't remember what you said. If we have ideas, whom would we see?

DR. SCHILLING: Whom would you see? Well, as in the past, I want to explain that Dr. Roman is head of our Observational Astronomy Program. This is one of the projects where she has administrative management and responsibility. Participating in this project from the NASA engineering scientists, our Ames Research Center. They have prepared the presentation. Some of you have visited them out there.

Just come and see Dr. Roman. I think your question was really related to the immediate interest for study. Any inquiries you have, please address them to Dr. Roman. If you have project proposals just address them to National Aeronautics and Space Administration. They will all go to our Research and Contract Office; Dr. Lloyd Wood will receive them. They will again end up with Dr. Roman. It is her responsibility and her associates to make decisions.

DR. ROMAN: I should like to repeat what I stated earlier, though, that I think all of you will be hearing from our procurement office, I hope in the not too distant future, which will give a few more details on the process of submitting proposals for study contracts in these areas.

MR. GILBERT (Bendix Corporation): Will these be on a competitive basis, or will these be a sort of solicited list?

DR. SCHILLING: The major procurement items as well as small items, regardless of funds, will be on a competitive basis. In fact, within the Astronomy and Astrophysics Program all the basic research contracts that have been let so far have been competitive. Believe me, we have had hundreds of proposals. However, when we talk about straight procurement items as against basic research aspects, we talk about formal bidding procedures.

DR. ROMAN: I think I would like to restate what you have said in a slightly different way. I am saying the same thing. Any proposals which you submit to us are necessarily going to be judged on a competitive basis. If Bendix submits a proposal and Ryan submits a proposal for the same thing, we are going to look at it and decide, well, who looks like they are going to get the best results and who is going to do it for the least money. I don't think we are at the stage where we can give you a firm set of specifications and go out to the type of bids where we want the lowest price on producing 10 packages with very detailed parts to them.

DR. SCHILLING: Since funding was mentioned, this list which you read, Dr. Roman, this does not talk of millions of dollars?

DR. ROMAN: No.

DR. HELVEY: What kind of ceiling would you say? You said not a million dollars. $100,000? $50,000? $10,000

DR. ROMAN: I ~~think~~ it depends largely on the contract. I think the numbers you have been mentioning are in the right ball park -- not the million dollars.

DR. SCHILLING: In the present fiscal year the funds are very much limited. We are really talking about something like 10 to 50 thousand dollars in one subject area as against the other, which is really something to get started in research development along this line.

Dr. Roman, you have two films which will be shown, I understand.

DR. ROMAN: Would you like to see a film on the air floating platform or not?

DR. SCHILLING: Since also earlier this morning there was mentioned a study being done by Dr. Meinel for the National Science Foundation in basic research aspects of astronomy in space, is Dr. Meinel in the audience?

Perhaps if you could come up here and describe in a few words how this fits in? I want to mention that we have in existence our working group on orbiting astronomical observatories and Dr. Meinel is a member of it. Could you come up and describe in a few minutes what you are doing and how it fits in with the long range plans?

DR. MEINEL: Dr. Schilling, earlier today, in fact starting the presentation, Dr. Roman mentioned two reasons to go above space. All the experiments that have been described involve one of those reasons only. That is the fact that you are above the selective absorption of the atmosphere. The other one, above the turbulence, leads to another aspect that is the high resolution one.

Rockets have gone above the atmosphere, and have explored the ultraviolet section of the sun. Also balloon telescopes have gone high in the atmosphere to gain resolution. As in the case of the rockets, the balloon has only an occasional glimpse. Repeats are slow in between and they serve as, again, rockets to whet the appetite of scientists to know things a little bit more on a continuous basis.

Forgetting the fact that you need a vehicle, we are examining the question of just what would meet both requirements. This has led us to a size, a 50 inch apperture as being the smallest size telescope that would permit a significant gain in angular resolution over earthbound telescopes. This sets a rather large size and involves quite a bit of weight and is out of the question as far as the vehicles that we have been talking about today are concerned.

We also considered what would be the most desirable orbit to put this in and came to the conclusion that the 24 hour orbit has many things to commend it. You suffer quite a payload penalty to get to that altitude, you must admit, but such things as gravitational torque are less by factors of the order of a hundred or more, reducing the disturbance problem from the gravitational torque. Your gain outside the Van Allen zone is roughly the same level you would get in some of the low altitude orbits. You still have the radiation pressure problem to bother you. You are out of the sputtering problem entirely. You have a continuous access problem so that there is no data storage involved. In short, you admit to the payload penalty, you have benefits from there on.

This type of project is -- Dr. Schilling refers to the one discussed today as the second generation for third or fourth generation. Nevertheless, some of the problems appear to have such a magnitude that some study has to be done reasonably early. That is one of the reasons why we are exploring it. As far as vehicle goes, there are vehicles on the horizon such as the Saturn that have the payload capability for 24 hour orbit that is required by this astronomical requirement. In our detailed design we have departed a few places from the philosophy of the present one. One is in our star acquisition and guidance system. We have one advantage which I can show graphically by the globe here.

We are off 6.6 earth radii so the satellite is somewhere in this position with respect to the globe. As a consequence, we can select guide stars which are never occulted by the earth or moon. As a consequence, we can have continuous acquisition and guidance on the stars, and the system that we will envisage actually more of a navigational system which picks two bright stars and operates on them almost continuously.

The method of acquisition is simply that once you have oriented the sun, you can set off a definite angle, as Dr. Code is proposing, and simply pick up the first bright star. Then you can set the second angle to pick up your second star. You triangulate your coordinate system and remain on this. As long as the vehicle has orientation position, you can select your stars and maintain these and observe some 90 per cent of the sky without moving off the stars. So it simplifies some of the guiding problems. Of course, the continuous acquisition if this can be operated means that you can use fairly large dishes, you don't need 60 foot dishesto slew at high angular rates to keep on your target. You can only take account of the small perturbations produced by the equatorial bulge and the moon on such an orbit. We are exploring some of these problems.

The moment you get to something as big as 50 inches there is a fundamental question as to whether you can ever expect to put a telescope of that size in orbit with anything approaching the theoretical resolution which is one of the reasons why we wanted to get up there in the first place. So there are many problems and many steps involved.

This study will go on and explore some of these solutions. I think there is mutual benefit in some of these systems. The point I mentioned to Dr. Triplett about the problem of being able to move in a coordinate system from one object to the other. In our case we have the same limitations, we put the inertial absorber on the navigational system. So you never change your inertial absorbers at all. So you get rid of some of these minor problems. The techniques of setting the telescope are different. You simply measure shaft angles with respect to your navigational system to your main package.

These methods are used conventionally. The question of shaft digitizing is rather standard process so that it involves a different type of acquisition. Of course, the accuracy with which you can fundamentally set the telescope is still limited to perhaps three minutes of arc. But this is usually close enough to pick up the object you wish to study in your prime optical system. So it perhaps has an advantage from that standpoint.

Now, there are undoubtedly another bunch of questions coming up since the radiation torque is not changed. In our preliminary design work we hit a rather dfficult problem. For instance, just simple configuration wise, because the solar cells are essentially black, the rest of your vehicle will probably be white because of thermal problems. If your radiation pressure is balanced on the center of gravity in one orientation, you change it, you shift the center of gravity, and you develop a new torque. It seems, offhand, about the only thing that is really independent of this is a white billiard ball covered with freckles, and the freckles are solarcells. Of course they are not working at maximum efficiency, but it is the only configuration we can see that you have any symmetry.

I think this outlines a few of the things that we have been looking at with the view that some day the capability may exist and need may exist for such an instrument.

DR. SCHILLING: Thank you, Dr. Meinel. May I add hopefully, maybe three years from now when the problems which we heard about this morning are all solved and the vehicle approaches a count down stage, we will have a similar meeting here where we will discuss what to do next.

Are we ready for the film now?

DR. ROMAN: Maybe I can take this time to make one announcement which I was asked to make. It has nothing to do with our program today. Dr. Helvey, of Radiation, Inc., has asked me to call your attention to a symposium on space trajectory which is being sponsored by ARPA and the American Astronautical Society, being held in Orlando, Florida, in mid-December. For details I suggest you see him. You had better stand up so that people will know what you look like.

Are there any other questions while we are waiting?

QUESTION: If by some stroke of luck someone should propose a novel idea for a development program, would that idea be put up for competitive bids, or can a negotiated contract be arranged?

DR. ROMAN: I think I see what you mean. I think the question we were answering before was a little bit different from that. I think at this study contract phase we would consider that the competititon was in the ideas. It does not mean that having been given an idea, we would then take it and send it out to industry for bids.

DR. SCHILLING: Any more questions?

MR. BURNELL (Aeronautical Lab): With respect to this 24 hour orbit, have you considered, or would you care to say something about the problem of keeping the satellite over the part of the earth from which you can see it over a reasonable period of time?

MR. MEINEL: We have considered that and ceased to consider it when we were simply told that they had to achieve this capability for other problems of far greater importance. Therefore, we assumed that it would be achieved. That is an easy way out.

DR. SCHILLING: Dr. Roman, it looks like we might get one of your observatories up before we can get a projectionist.

DR. ROMAN: Mr. Gillespie, from the Langley Research Center. He owns one of the films which we are waiting to see.

MR. GILLESPIE: The Space Orientation Control Programs at the Langley Research Center, I believe in our preliminary work have come up with two results which may have some application in this particular space mission. Item No. 1, we have been working on a design for a solar sensor which makes use of silicon cells mounted at an angle which would be oriented to the direction of the sun. These would be connected in a bridge circuit and their output of course then would be working against each other to give a control signal. The configuration we have drawn or pictured on the first slide.

(Slide)

I don't know if this is visible to the people in the back. On the left side we show what we call some coarse sensors in two arrangements, one with an indented or inverted pyramid around triangular cells which in this case would have a capture capability of approximately 90 degrees. In the bottom case we have raised these cells up above the surface of the vehicle which in this case is not a flat surface, but which continues the slope of the silicon cells and increases the angle of capture to approximately 150 degrees. For this case the cells are mounted 60 degrees from what would be the normal flat surface. Over on the other side we show that by adding a shield, the shield is a long rectangular column, we can increase the sensitivity of this arrangement by the shadow effect from the shield.

For long duration operation in orbit we have indicated a possible scheme for correcting any aging effects which would tend to change the characteristics of the individual components. However, we are uncertain as to how much change may occur and just how big the problem might be.

In the next slide we show the measurements made of the voltage output of the coarse and fine sensors which we described. In this particular case the source of light was an aircraft landing light of rather low intensity compared to the sun, and each cell was really made up of six individual solar cells, which is the reason for the rather large voltage output. But this large voltage output is one reason for going to the silicon cells. That is, we feel there would be less amplification required in that actual final design.

The curve in red here would be for the coarse sensors; by adding the shield we can improve the sensitivity by the shield. I think that will be enough for the slides.

DR. ROMAN: Do we have a projectionist for the movie now?

MR. GILLESPIE: I think I could probably sum up what the movie indicates. We used a preliminary version of this solar sensor in connection with the simple solenoid operated air jet system, cold nitrogen, and the pointing accuracy achieved to date has been plus or minus 18 seconds of arc. To do better than this in the ground simulation test made with the use of the hemispherical air bearing would seem to require some attention to reducing the traditional background noise from the room. People walking around the room and air currents and the like. But we feel that this solar sensor can be developed along with a more refined control system to get down to the one second of arc pointing accuracy which has been mentioned earlier.

The other thing which the film shows is one of two methods we have been looking into for means of unloading the flywheel to prevent saturation. Both methods that we are considering would make use of the reaction we can obtain with the earth's magnetic field. In the film we have made tests with a combination system of a flywheel working along with a bar magnet, with the bar magnet giving a capability for reducing the flywheel speed.

A second alternate method which we have not yet tested but which looks feasible for the case where the inertials of the satellite are closely equal -- instead of designing the flywheel in the form of an elongated cylinder, to maximize the magnetic skin damping effect that can be obtained by a spinning body in the earth's magnetic field. This seems like a possible solution.

I think if we have the film, we can then start. The film has titles which are somewhat self explanatory.

DR. ROMAN: I would like to suggest that since there has been the difficulty and the delay in the film and I know that at least some of you have other appointments to catch, that perhaps we should adjourn the meeting, and then those of you who would like to stay to see the two movies are perfectly welcome to do so.

Before I adjourn I have a message here for Mr. Reichenbach of Kearfott Company, if he will see me. I want to thank you all for coming. I am very sorry I kept you waiting this afternoon, but I appreciate your loyalty in waiting. Back at college it used to be you waited ten minutes for a full professor, and then you left. So I was rather startled to see a room full of people here when I returned. However, thank you, and I imagine I will be seeing you again. I know you will be hearing from me.

(Thereupon at 3:15 p.m., the meeting was concluded.)

Figure I - Milligan - Goddard Space Flight Center

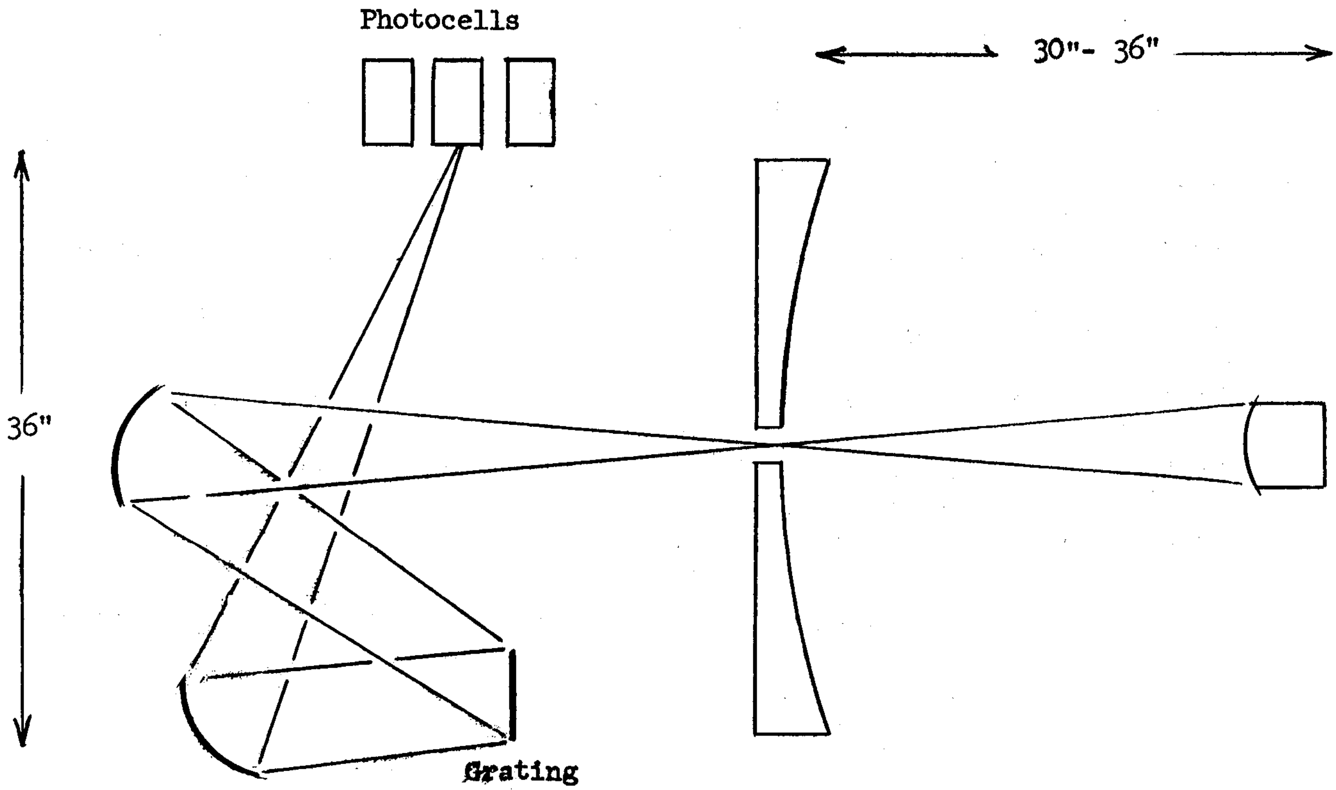
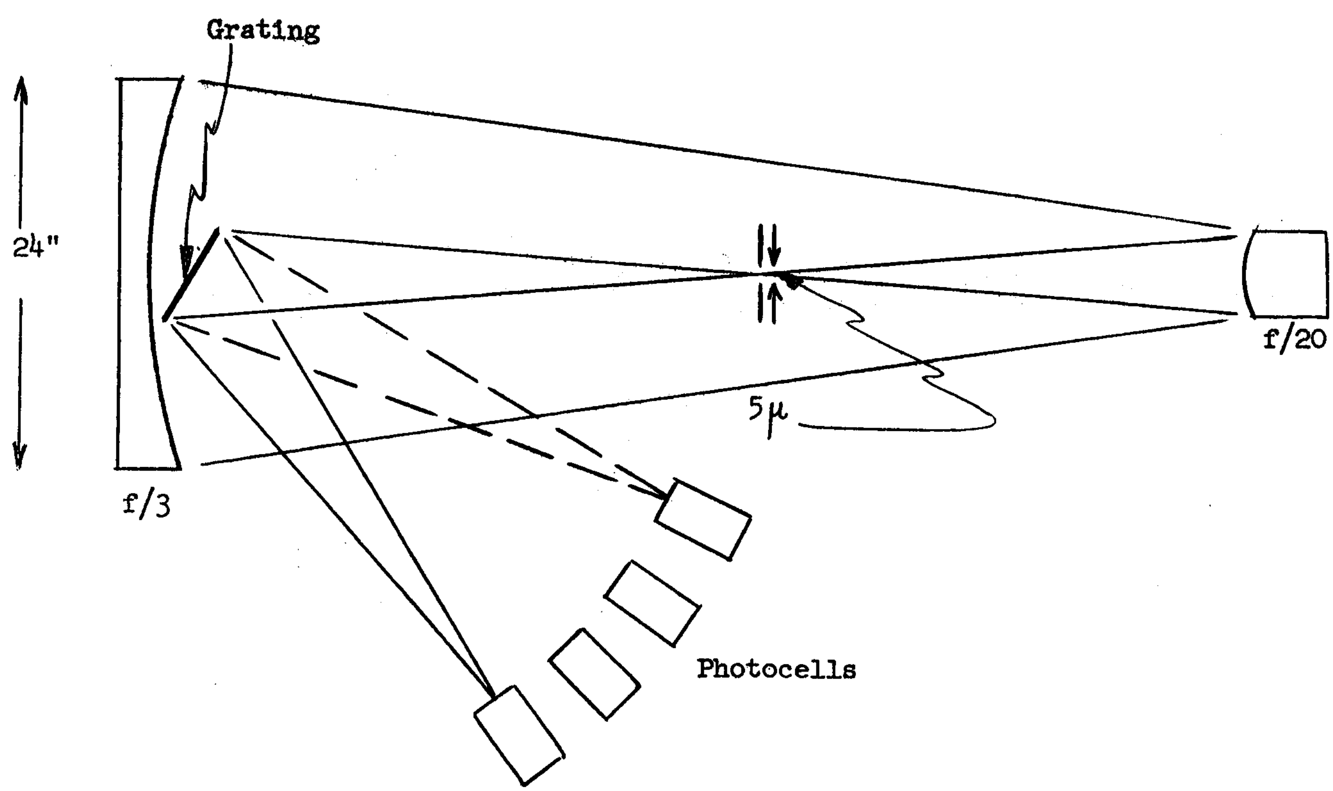


Figure 2 - Rodgerson - Princeton University



NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

Tuesday, 1 December 1959

PRESS CONFERENCE ON LUNAR SCIENCES

The press conference was called to order at 4:00 p.m., Mr. Herb Rosen presiding.

PRESENT:

MR. HERB ROSEN, presiding.

DR. ROBERT JASTROW, Goddard Space Flight Center, Chairman of the Lunar Science Group.

DR. HAROLD UREY, Scripps Institute of Oceanography, University of California, La Jolla, California.

DR. THOMAS GOLD, Cornell University, Ithaca, New York.

DR. HARRISON BROWN, California Institute of Technology, Pasadena, California.

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MR. ROSEN: Ladies and gentlemen Welcome to what we loosely call a press conference.

I should tell you why we are having this. It is a rare and fine opportunity that we have presented to us to bring before you people such as Dr. Urey, Dr. Gold, and Dr. Jastrow, to discuss the thing that is really foremost in our minds -- that is, the moon.

This is the reason for holding the press conference. But the reason for bringing them into town is that these gentlemen are members of the NASA Lunar Science Group. Yesterday they met with other NASA people on what is loosely known as a conference on the moon. Bob Jastrow tells me that the reason for this is to try to explain to the geophyicists and geoscientists that there is a relationship between their problems in the exploration of Earth that are common to the problems concerned with the exploration of the moon.

The Lunar Science Group of the National Aeronautics and Space Administration has as one of its most important functions the business of providing NASA with close contact with eminent scientists interested in lunar science, and conversely to provide the scientific community with an assured contact with NASA.

The Group will be an invaluable source of ideas and recommendations as it has been in the past for the continuing lunar sciences program of the National Aeronautics and Space Administration. It will be able to assist the Office of Space Science in defining scientific objectives, payloads, and support requirements for a sound national scientific lunar program.

For those of you who are interested in the membership of the Lunar Science Group, it consists of the following: Gordon MacDonald, University of California, Institute of Geophysics, Los Angeles; Harrison Brown, California Institute of Technology, Pasadena; Harold Urey, Scripps Institute of Oceanography, University of California, La Jolla, California; Morris Ewing, Columbia University, New York; A. R. Hibbs, Jet Propulsion Laboratory, Pasadena; Ernest Stuhlinger, ABMA; Thomas Gold, Cornell University, Ithaca, New York; Bruno Rossi, MIT, Cambridge; Frank Press, California Institute of Technology; Joshua Lederberg, Stanford University, Department of Genetics; and the Chairman is Robert Jastrow, of the Goddard Space Flight Center.

On the platform we have, from my right to left, Dr. Gold, Dr. Jastrow, Dr. Urey, and Dr. Harrison Brown. Later on perhaps Dr. Rossi of MIT will be in.

Gentlemen, you are here before the press. They are to glean from you your words of wisdom. We are going to transcribe this conference and the words will then be passed out to a large number of newsmen. Material that will be contained in this transcript will be used for valuable background for later articles that will probably appear. For you ladies and gentlemen, a transcript will be available some time tomorrow morning.

Dr. Jastrow will open the conference and give you some highlights of the conference we had yesterday with the geoscientists, then remarks from each of the participants, and then we will throw the discussion open to you on the floor.

DR. JASTROW: If possible, I would like to propose that we throw the floor open for questions after I make a few remarks to the members of the Group.

MR. ROSEN: One other criterion. The subject is the Moon. There will be nothing on payloads, nothing on programs with regard to when something is coming off, and that is the principal reason I am here.

Dr. Jastrow?

DR. JASTROW: We are very pleased to have the opportunity to talk with you and answer your questions.

With reference to the conference we had yesterday, it was sponsored by NASA. It was held specifically by the Theoretical Division of Goddard. It concerned itself with the problems of lunar research, not the money and management problems but the scientific problems, the questions to which we would like to obtain answers, why we care about the answers, and in yesterday's conference to a very limited degree how we propose to go about getting them.

In this conference, which was run in a very informal manner, the morning session was devoted to some discussion of the meteorites and their origin. These are small bodies, some small, some large, which fall on the surface of the Earth, and there is good reason for believing that some of them at least come from the moon. There is a great deal of dispute about this last point. That dispute was interwoven throughout the discussion yesterday.

Dr. Urey in particular discussed his ideas on the origin of the meteorites. I think he would be delighted to talk to you about that point as well as about his very strong interest in the moon itself, apart from the possible fragments that it rains on us.

Following the discussion of the origin of the meteorites and some discussion of their possible relation to lunar matter, Harrison Brown presented the results of an analysis of the distribution of sizes and weights of the meteorites and showed that on a log plot. This distribution was a straight line, with slope 0.77, which when extrapolated to very large masses indicated a yield of major fragments, major meteorites, such as might have produced the Arizona meteor crater, and that which fell in Siberia in 1908, I think.

If I am not mistaken, he mentioned to me at one point that on this extrapolation such large falls were about ten thousand years apart. He may wish to say something about that, or some other aspect of his research.

Harvey Allen was also present and talked to us about some re-entry studies based on his ablation work and related to the ablation of meteorites in the atmosphere.

John O'Keefe talked about the tektites and the general problem of the meteorites.

Gordon MacDonald, who unfortunately has had to catch a plane back to the Coast, talked about the interior of the moon, its thermal history, that is, how hot it was during the course of its 4½ billion years or so of lifetime, and the likelihood of internal seismic activity, moonquakes, et cetera.

Then there was a discussion by Dr. James Arnold, from Scripps in LaJolla, of the detection of lunar radioactivity, which again for reasons I hope may come out in this discussion appears to be one of the most important if not the most important first experiment that one might conduct in the vicinity of the moon.

Finally, Thomas Gold discussed some of the ideas current on the properties of the lunar surface, the nature of the mare, et cetera.

That concluded the day's session. I think all parties concerned enjoyed it very much. There was a great deal of very loud debate and bitter wrangling which we carefully preserved on tape and hope to be able to have you hear and other people hear some day, if it is reproducible.

That is all that I have to say. I would like to throw it open to the floor.

MR. ROSEN: Are there any questions?

QUESTION: Dr. Jastrow, you said that you discussed questions to which you would like to obtain the answers, and why you would like them. Would you enumerate these for us?

DR. JASTROW: Yes. Let me ask Harold Urey whether he would first comment on why we would like to have these answers, why we think the moon is an important object to science.

DR. UREY: People have been interested in the history of the Earth for a long time. It is an interest that concerns a great many people at the present time. People have been much interested in the origin of the solar system, not only the lay interest that we are all acquainted with, but because the development of the solar system is an example of the development of stars, of which there are many.

If one looks around to find places where the record might be preserved, I believe that there are a few places where this record of the history of the solar system is very well preserved in several ways. The Earth is rather bad, but valuable in many ways. It is bad because there is running water on the Earth which has changed the surface of the Earth and given it a very interesting history during the last billion years or so. But this running water has destroyed the more ancient record.

The moon has had no wear on it at all and hence the events that have occurred on the moon are recorded there since the time that they were produced.

The meteorites are another sample of matter in which a record, a very ancient record is preserved. We know from them that it is about $4\frac{1}{2}$ billion years since a great many of the processes that produced these objects occurred.

We are fully expecting that the surface regions of the moon will give us additional information about a very ancient record of the solar system.

And so this in itself will enable us to answer some interesting questions. For example, was the moon ever completely melted? If it was completely melted, then this requires that we had a very high temperature process a long time ago that produced the Earth. Or if it was not completely melted, was it partially melted, and in what ways was this produced? Things of this sort.

Also, I am much interested in it from the standpoint of the abundance of the elements, what is the fundamental abundance of the elements that the physicists are trying to explain. We practically exhausted all sources of material that was of interest to us, namely the meteorites. No, there are a lot of things to know about the meteorites, but at least we are feeling the need of additional samples. The moon will supply this in certain ways, we think.

There are other things. There are the asteroids which should have a well-preserved record as well. But the moon is nearby, and it has no atmosphere, no water. It is cold enough to preserve a great many of the things we wish to investigate, and so this is our first stopping place in our exploration of space.

Does that suit you, Bob, as a beginning, let's say?

DR. JASTROW: Yes. Let me add three sentences to it.

MR. ROSEN: We might also have rebuttal.

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DR. JASTROW: This question of the origin of the solar system I hope does not seem to you to be a dry and very academic one. It is a question of tremendous interest and unexpectedly wide-spread importance.

There are a couple of ideas that are current at least in recent times regarding the origin. One of the popular ones awhile ago was that the planets and other bodies in the solar system were torn out of the body of the sun by gravitational forces in a near collision with another star going by by accident, and that these hot masses of gas condensed in the course of time in the planets and satellites as we know them.

Another theory holds that the sun itself was formed by the condensation of interstellar gas and as the sun condensed into a disk and under gravitational attraction to a more compact mass, there was some mass left around the outside of it which in turn condensed into smaller centers, and those are the planets.

Not the planets but at least small objects which may have been of planetary size, or may have been smaller, and they collided in the course of time, some of them, and because large masses attract more strongly than small bodies, there was an instability and accidental fluctuation could produce very large bodies by the secretion process.

The point I want to make is that if the solar system was formed out of hot gas of the sun, then surely the bodies in the solar system were at one time very hot, and the moon and earth as they condensed must have been in melted form before they solidified.

On the other hand, if the condensation occurred out of cool gas by accretion of small objects then it is very likely that they were never melted unless radioactive elements in the interior raised the melting point. We will also have learned a great deal about the probability of life about other stars which is a subject of tremendous philosophical and scientific interest.

The probabilities of collision between two stars is very remote. On the other hand if the solar system was formed by the condensation process it is much more probable that there are planets around a great many stars.

Of those planets, the ones which happen to be of about the size of the earth and about the right distance from their stars to get the same amount of radiation as the earth, will have circumstances favorable for the creation of life as we know it.

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This is all in the background of this problem of determining the origin of the solar system.

As Harold has said, in that problem the moon is the most interesting object in the skies for us, nearby object, because its history has been preserved throughout its life of three or four billion years.

DR. GOLD: Of course I completely agree with what has been said about the importance to the understanding and origin of the solar system that the moon gives us.

The ideas that one has about how many other solar systems there might be, of course is very much dependent upon the correct interpretation here. One has reason to believe that perhaps all slowly rotating stars are rotating so slowly, otherwise very difficult to explain, are rotating so slowly because their spin has been lost through friction with a disk of material giving rise to planets such as is one of the more modern theories of the origin of the solar system.

In that case one would think then that all slowly rotating stars are stars possessing planetary systems or at least disks of gas that are capable of condensing into planetary systems.

In addition to seeing the details of the geological record embeded in the moon, of the geological, as it were, lunological record of the solar system, to see that embeded there, in addition to that one might also discover facts about the origin of the solar system from the dynamics of the gas which is still in existence now and which might give us a clue about the gas dynamics that might have been responsible for the actual formation process and the distribution of the momentum in the system.

Would you like me to say something about the moon?

DR. JASTROW: If you hadn't I was going to say now let's get to some of the things that we really got hot about yesterday, namely, what is on the surface of the moon, how do we think it compares with our old ideas. What do you think?

DR. GOLD: I have been proposing the point of view that the moon itself has been through most of the history, that the present features on its surface relates to, through most of its history it has been cold or largely cold, cold enough so that no very large changes of internal origin have occurred.

The arguments for this are that we see a lot of markings, as you see on these pictures, of roughly a circular sort.

DR. JASTROW: I know this won't be on the transcript. Dr. Urey got us interested in the moon and its particular importance sometime ago -- a year or so ago. When he first showed us his graph of the moon we saw very little on it. It took a year of looking at it to really see all these things.

DR. UREY: It always does.

DR. JASTROW: I would like to make sure that you get the points that are involved.

For example, Tom points out that every one of the markings on this much-scarred surface is an exact circle. You don't find any displacements produced by mountains.

DR. GOLD: When you say an exact circle, it is not that it is precisely circular, but no way has it shifted, deformed, substantially pulled out of line. While if you took any markings that you had put a long time ago on the earth's surface, a lot of markings staked out with stakes, if you like, and come along a billion years later, there would be very few places on the earth's surface where there would not have been large contortions of the ground that would have displaced this very greatly.

DR. JASTROW: Isn't it correct that on the earth such configurations might last about ten to a hundred million years?

DR. GOLD: Yes.

DR. JASTROW: If not longer. It is a short time.

DR. GOLD: Not only for erosion. Quite apart from erosion, for reasons of internal deformations that are impressed upon the earth's surface, these deformations are conspicuously absent on the moon.

DR. JASTROW: That means that everything here is three or four billion years old.

QUESTION: How do we know that?

DR. GOLD: We have the following reason: We are pretty convinced that all the pock marks that you see are due to impacts with quite large objects. There is no other reasonable way of interpreting the detailed features, and indeed the actual even present-day rate of bombardment on the earth by substantial pieces would suggest in any case a certain fraction to be due to that process. And since they all look much alike, we therefore suppose are. If they are due to impacts, then they must be due to impacts that occurred at the time that preceded, at least for the bigger ones, the bulk of the geological record on the earth, because otherwise the earth would show that kind of a surface.

That alone puts it a matter of more than about a billion years ago. Otherwise we would go to the Canadian Shield and we would find it all full of circular holes just as any region, for example, here, would be.

But it isn't. These holes are quite deep. Despite all erosion they would still show in the geology of the ancient regions of the earth. They are not there. There might be some there but they are not there in this kind of concentration.

So this puts the whole story back, that the bulk of this bombardment must have been only about a billion years ago.

DR. UREY: Three billion.

DR. GOLD: In fact one would be inclined to put that date substantially further back. But I think one could really sort of swear that it must have been over a billion years ago, most probably something like three.

QUESTION: What kind of a mark would an object hitting the moon at an oblique angle leave?

DR. GOLD: It also leaves a circular mark, or roughly circular. The point is that objects hitting the surface at a very high speed do not make an impact hole like a bullet would at a much lower speed because the bullet's hole is formed chiefly by pushing the material out of the way by the impact.

At these very high speeds the hole is made not by that process but by the fact that the incoming object is so intensely heated by the collision at this very high speed that it is vaporized and a considerable quantity of ground is vaporized, therefore the essential hole is made -- a large part of the hole is made just from the sudden release of this extremely hot gas, a large quantity of very hot gas that is suddenly released. That makes a circular hole, roughly speaking, a hole whose shape at any rate is not very dependent on the direction at which the piece came in. It is just that at those speeds any solid behaves like an explosive at impact.

QUESTION: What sort of a swarm of material might we have run through a couple of billion years ago to create a situation like that on the moon?

DR. GOLD: Some of us think that that is the swarm of material that was really part of the origin of the solar system material. That what you are seeing here is perhaps the tail end of the material to be picked up to form the planets. Some of that very tail end perhaps is still in existence now as meteorites.

QUESTION: And as asteroids also?

DR. GOLD: And as asteroids. There is no doubt that if you waited long enough now that the meteoritic material in the solar system and the asteroid material in the solar system is being depleted because it is still being picked up by planets, undoubtedly in much greater quantity at the moment than it is being shattered off them.

But of course one would expect that the final phase of such a process would take a very long time. By the time you have only a few pieces left, comparatively few, it will take a long time to gather them all up.

So sometime in the past the rates, presumably, were higher and were responsible for all this. Some of us think that this is the kind of surface which a body will have on it when it has just been put together from a certain distribution of large and small pieces.

One aim is to interpret the lunar features in this light, and with the great absence of any internal upheaval, with no mountain building process of internal origin but only with the pock marks being the only surface features, one is very tempted to think -- at least I am very tempted to think -- that there were no large internal re-melting processes, that there were no large lava flows, no great outpourings of internal material, because such outpourings of internal material one would think would tend to distort all the adjacent regions, distort the ground and buckle it and contort it, as happened on the earth with much smaller outpourings that have on occasions been suggested.

DR. JASTROW: Don't these look like lava?

DR. GOLD: The argument has usually been the following, and this is the one that I oppose: That all the dark regions that you see, which are remarkably flat, though not quite flat, that all these regions were caused by flooding by a very fluid lava, fluid enough to distribute itself flat over big distances, and that therefore an enormous amount of volcanic activity must have occurred. This idea of course became current at the time when one still thought of the pock marks themselves being volcanos.

There is so much evidence against this point of view, of the pock marks being volcanos, that most people accept the point of view that these are impact craters but still hang on to the idea that the flat regions were lava of internal origin.

DR. JASTROW: How about that Russian observation of the parent volcano?

DR. GOLD: The Russians reporting a volcano, I would like to make clear that we believe that any volcano, any active volcano of a terrestrial sort, such as there are hundreds of on the earth, on the front face of the moon would have shown up in a way that we would have known about for very many years, we would have been very clear about it.

The quantities of gas, ashes, vapors of all sorts that come up from the interior of active volcanos, quite apart from the big eruptions, are so great that they would show up optically quite easily on the moon.

There is no question that the face of the moon that we see, and probably not the back, either, contains any, even the smallest of active terrestrial volcanos.

QUESTION: Then you think that Russian observation was an error?

DR. GOLD: I wouldn't say that. But I would say that it is quite possible, and really to be expected that from time to time some gas will blow out of the interior of the moon. That is a thing not necessarily related to volcanism. If you put a big object like the moon into a good vacuum and watch it for a long time, unless it keeps absolutely still, the slightest bit of cracking that occurs, the slightest bit of distortion, will release internal gases, of which there must be plenty locked up on the moon, and the quantities of gas coming out into this very good vacuum immediately may produce visible results such as they claim to have observed.

But it is quite certain that this was not an outburst in which big floods of lava came out or anything of that kind. It is not like a terrestrial volcano.

DR. JASTROW: I want to show you the crater in which the outgases were observed. It is unfortunately not on this half but on the half I left in Mr. Rosen's office.

We don't have the half with that crater. It is called Alphonsus.

QUESTION: Incidentally, Koserov says it is still going on. The Russian said the outgassing, or whatever you call it, is still going on, that he detected some since then.

DR. JASTROW: We discussed this question at some length in the symposium we had here in Washington last April. The consensus, I think it is fair to say, was that there may have been some outgassing, there was no violent eruption, and may indeed still be going on.

DR. UREY: I might remark, if we think a little bit about what it would be like, the outgassing problem would involve water, if relations to the Earth mean anything at all.

You have almost a perfect vacuum. And so this water would rush out, evaporate and go down to a low temperature and the water would freeze and finally freeze up the hole through which it was coming and it would still evaporate and you would expect that it would partake somewhat of the action of a geyser.

If you think of the geysers of Yellowstone, they have been going on, to my personal knowledge, for fifty years without making much change in the appearance of the geyser at all. And so this sort of thing happening on the moon would be one that would leave no marks that one would see at all. But if happened on the moon, nobody would argue about it at all. We would all be sure it was there.

DR. JASTROW: Before we go on to science, let me make a remark about the moon and the artist's conception of it, which I think is important.

Actually, if you look at this and find it very scarred and apparently consisting of these high mountain walls and surrounding flat plains, I must remind you that such a crater as this is something like twenty or thirty miles across -- I am not sure of the scale. This one would be fifty or sixty, Tom says. And around it there are what seem to be fairly steep ramparts. But actually they are only about ten or twenty thousand feet high above the floor of the crater. So if we were standing in the middle of the floor of this crater and looking around at the horizon we would see a rather gentle roll of hills. It would seem that way.

It must be remembered there is no atmosphere to give you distance perception. There is also a rather gentle hill. If you are standing on the lip of that crater you would see no precipice. The lip, I believe, rises only a few thousand feet above the surrounding surface.

DR. UREY: There is a very, very gradual mountain peak.

DR. JASTROW: With a few exceptions in the highlands the moon is not the conception of a flat plain with steep craggy peaks towering out of it. It is a broad rolling region. We don't know the structure. We don't know if it consists of broken lava bits or other things.

DR. GOLD: We know something about the fine structure, namely that radar observations have shown that it is remarkably smooth down to dimensions of the order of a few inches. It is not very much rougher on the scale of a few inches than the roughness we see on this kind of scale where we can see about a mile. That is to say, when we see a gradient extending over a distance of a mile here, when you go in detail and look at it, you will find it over that distance of a mile to be pretty smooth, not to be broken up into little ups and downs.

QUESTION: It would be a pebbly or sandy surface?

DR. GOLD: Yes, with the units of it being at any rate smaller than a few inches. It is sure from the radar observation that the surface is not all covered with boulders, shall we say. If we had thought of this being pock marks blasted out of very hard rock, and with nothing else happening after that, then you would do best to go to Northern Quebec and have a look at the crater which I am almost certain is meteoritic there, and it is about two miles across. It is composed of very tough rock with the pieces that were thrown out to make the big bowl, all pieces of this sort of size (indicating). And there is a thousand-foot wall, or something of that order, around this crater, and it is entirely through its whole volume, as far as we can tell, composed of chunks of rock that size, each weighing a matter of three or four tons.

QUESTION: Is that Chubb Crater?

DR. GOLD: Yes. If all this amount of bombardment had just occurred in hard rock like that, and were not subsequently eroded, then we would expect all this to be as rough as Chubb Crater is and the radar observations would be completely different. So there is no question that this is not the situation.

I think for this reason and for some others an erosion process must be envisaged as having occurred that works in the way of making things remarkably smooth. I personally think that it goes so far as to make the very large flat regions the filling in in a very flat way of fine particles that are responsible for the erosion. So that all this I think of as being sedimented powder.

QUESTION: Without wind and without water, where does your erosion come from?

DR. GOLD: How does the stuff get transported?

DR. JASTROW: One is the mechanism of transport of the fine material, and the other is how it is powdered.

DR. GOLD: One mechanism of the construction, such impacts made all these big holes. They are all round, so it is pretty clear those were large impacts. A large proportion, to say the least, of the material will have in any case have been made into fine powder then. So it may be that the problem as to what makes the fine powder isn't very hard. Almost all that we see might in fact be made of very fine powder, loosely cemented together in some places, but not much.

The transportation is the real teaser. It has seemed to me that by far the largest force to be expected to be active in moving any particle on the surface, on the Earth, would be wind and water. On the moon, in a vacuum, it is likely to be electrostatic forces. I can easily move the dust particles, as on the table here, electrostatically, by electrostatic forces. We have a perfectly good agency in the way of the sun, in the presence of a vacuum, we have a perfectly good agency for charging in an eratic small scale pattern the dust particles that lie on the surface. And I think that that provides a way of moving the material by charging it in such a way, eratically, that every now and then some particles jump.

QUESTION: Repel each other?

DR. GOLD: Repel each other and move. So that every particle on the surface suffers a little jump every now and then.

By that process -- I know that sounds like a slight process -- the time scales we have available are enormously greater than the erosion times available on the Earth, and if one makes estimates they are perfectly reasonable. Indeed, if one makes an experiment, as was done by a graduate student of mine at MIT, if one makes experiments with bombardment of rock powders with beams of charged particles, such as of the same nature as do come from the sun and undoubtedly hit the moon, one can reproduce this method of eratic jumping of grains.

So this kind of hypothesis that the flat regions are all made in that way, and are a denudation of the high ground, is the alternative to the point of view that other people have largely held, that all the flat regions are due to internally-supplied lava.

DR. JASTROW: At the beginning some one asked the question, why do we think this is important to find out about the moon. We said it is concerned with one of the most fundamental of all problems in human investigation, the origin of our solar system, and that has many ramifications.

Then the gentleman also asked, how do we propose to find out what is there. I wonder whether it might not be interesting to mention a few of the things that we have in mind. Not when we plan to do them, but what we plan to do.

In this connection, there are really two motivations. One is that one wishes to explore the surface of the moon in great detail to find out if it really is covered with dust as Dr. Gold suggests, and it surely is, and it is only a question of how deep the layer of dust is, and how big the rocks are, and what the fine-scale structure of the surface is so that we can pick good landing sites eventually for depositing packages of remote-controlled instruments. That one would hope to do from a television camera or photographic instrument in a satellite, a lunar satellite. It will take a while to do that and do it well.

Another point that we want very much to examine, and this is an experiment which is being developed by Dr. Arnold -- who is not here, unfortunately -- is the detection of the radioactivity in the surface of the moon. The reasons why that is important are twofold. One in particular. We want to know whether the moon has ever melted at any time, and what its thermal history was, because that is related to the origin of the solar system. How hot was it at earlier times in its history. That heating is to a large degree contributed to by the decays of the radioactive elements, uranium, and so forth, in the surface of the interior of the moon. If we can measure the amount of radioactivity on the crust, we will have some idea of the concentration on the moon and have some idea of its thermal history.

In addition, different kinds of rocks and matter that have been proposed for the surface of the moon have different amounts of radioactivity in them. So if we measure the gamma rays which come off radioactive decays, we get some information about the surface composition.

Then finally, cosmic ray nutrons are captured by the surface, as Dr. Arnold and his collaborators have discovered,produce artificial radioactivity, and the gamma rays in the resultant decays, which again are characteristic of the elements on the surface.

So if we can put a particle, a gamma ray lunar detector on the satellite, we can detect something about the surface composition of the moon which we want to know very much before we land there.

QUESTION: Were you able to get any information on that from the Soviet moon shot, on the radioactivity?

DR. JASTROW: No, I don't think they had an activity detector.

QUESTION: We asked them that at the press conference, and they said they did have. They said 1/100,000 or 1/10,000 of a natural radiation background. The scientist, their cosmic ray scientist, stated that they detected nothing with this instrument.

QUESTION: Were they talking about --

DR. UREY: That is a curious thing. I was told that by the reporter of Business Week, and I have never been able to get a confirmation of it since.

QUESTION: Krassovsky made this statement at the press conference.

DR. UREY: I think it would be a good idea to write to them and see what they tell us they found.

I will do that tomorrow, Bob.

QUESTION: I don't know if they were talking about radioactivity or the radiation belt.

DR. JASTROW: We thought they had been misquoted and meant the radiation belt. I gather you were specific on this point.

QUESTION: We pressed them closely.

QUESTION: I think Irvine Hersey was having a transcript made of that press conference. You might write him at the American Rocket Society.

DR. UREY: Give me the name and I will.

Let me explain just a little bit about this radioactivity in more detail so that you will get the picture. Constant melting throughout the Earth has been occurring throughout its history. This material flows to the surface of the Earth as basalt, granite, and so forth. Uranium and thorium are increased in these materials by a factor of about one hundred or more over that that is found in the average meteorites, the most numerous group of which are the chrondrites. Potassium is increased in these surface materials by twenty to forty times.

If we turn to another group of meteorites, the achondrites, the uranium and thorium are increased by a factor of about ten over that of the chondrites, but the potassium is decreased.

Suppose that the surface of the moon has been differentiated by the Earth. Then we expect to find similar amounts of radioactive materials on the moon's surface. Suppose it has not been differentiated at all. It would be probably similar to some of the chondrites. That is very low concentration. And it might be intermediate if some melting processes similar to those that seemed to have occurred in the case of the achondrites were effective on the moon. So by just spotting this radioactivity we have a very quick analysis that indicates what kind of differentiation has taken place, and it aids us in understanding the history of the moon.

QUESTION: Dr. Urey, how much of this observation can be indirect or by inference, and how much actually is reduced to a matter of going up there and picking up a piece and bringing it back to analyze it?

DR. UREY: On this radioactive thing, by flying a vessel, a vehicle a hundred miles above the surface of the moon, say, or something of that sort, it should be possible to find out whether potassium has increased in concentration on the surface of the moon or not, and it is entirely possible that the Russian scientists have already made that measurement. We won't say they have, but it is possible that they have.

Now then, understand this of course would only tell us what is in the very outside surface of the moon. If there is debris and broken up material scattered all over the surface of the moon, maybe we are only looking at the debris and therefore we should be sceptical of the result.

Now then, if you wish to go farther than that, we must land instruments on the moon that pick up samples of the moon at the surface and down below the surface and at various places on the moon and make analyses of this kind.

QUESTION: Actually, we will have to do some mining there, then?

DR. UREY: Probably not deep mining, but one would want to dig away a matter of feet, ten feet or something of the sort, to get down to see what there is below.

DR. JASTROW: Harrison Brown has an experiment he is interested in and has been consulting on in the development of which involves the examination of surface samples. Would you say something about that?

DR. BROWN: I just think it should be emphasized that in principle it is possible to send instruments to the moon which will carry out chemical analyses for you by remote control, and where the resultant analysis can be telemetered back to Earth.

There are many kinds of approaches to this. The radioactivity experiment is one. Another approach which we are using quite successfully on meteorites at the present time is known as X-ray florescence, where you put a slab of meteorite under a particular kind of X-ray machine and you measure the radiations that are given off. You can actually determine the relative numbers of atoms of certain critical elements. Were we able to put one of these, to miniaturize it, get it to the moon and put it on the Earth's surface, we would be able to get an analysis with respect to these elements and in that way determine something about the chemical nature of the rock which is there.

In like manner, it is possible in principle to carry out another kind of analysis which we call neutron activation analysis, which we have used rather successfully for trace elements in meteorites. This would mean dropping a suitable neutron source on the moon with suitable detectors measuring the radiations which are emitted.

At the present time there are several of us who are interested in these problems and are working on them.

DR. UREY: May I just mention, continuing what Dr. Brown was saying, that very interesting studies have taken place during the last ten years due to the efforts of a number of very brilliant young men in this country -- in Washington, Chicago, California -- and also in the USSR -- directed toward understanding the ages of these objects by radioactive dating.

One of these methods depends upon the rate at which potassium turns to argon. The maximum date observed so far for stone meteorites, to which this applies, is 4.4 billion years. Since that time this object has not been warmed above ordinary temperatures, certainly that exist on the Earth, for all that length of time.

We would like to know the same thing about the moon. Was indeed the moon made at the same time that the meteorites were made?

This date of the meteorites is what we know. An inference that the Earth was made at the same time is only an inference. We have no measurements that certainly prove this. And the same is true of the moon. Was the moon made at this time? Are we indeed dating the actual process of the formation of the solar system in these studies?

Another interesting subject that has been investigated is the existence of certain gasseous materials that are produced by cosmic rays in the meteorites. This has been investigated in iron meteorites and stone meteorites. The oldest ages so far found by this process run to the billions of years as a maximum, though most of them are in the neighborhood of millions of years.

This lower age we think is due to the objects being broken out of a matrix that protected them against the cosmic rays. And the date at which they were broken out is what we think we are measuring.

Curiously enough the stone meteorites are much younger than this on the cosmic ray scale, some tens of millions of years. We are most puzzled as to why this should occur. There have been several suggestions made about this. I made one myself last April. Various suggestions have been made to account for this.

So far as the moon is concerned, if we can get a sample from somewhere around the poles of the moon, not too far below the surface -- we must stay pretty close to the surface -- and if we can have a sample of that material, can extract these gasses from it, and measure the quantities, we will be able to say whether cosmic rays are constant in time during the history of the solar system or not. At least we will have a total effect for the life of the moon, the age of the moon in regard to this problem.

We are much interested in these exceedingly powerful cosmic rays, where they come from, how they are produced, have they been constant in time and so forth and so forth. If you wish to go back in time on any of these problems, then a sample of the moon gives us an opportunity to investigate it.

DR. JASTROW: The interesting thing is that the moon is more important than Mars and Venus. The moon is the only object that we have accessible to us which has not been destroyed by erosion, surface destroyed by erosion. Mars, Venus and Earth all have an atmosphere, and possibly oceans at one time. But the moon is the only one that has all of its history written on it. And therefore if we want to know the origin of the Solar system it is the moon that you have to go for. I think that is the basic reason why we as scientists are so interested in the moon.

QUESTION: I want to ask Dr. Gold about the Soviet moon photo, if it has given him any new ideas.

DR. GOLD: Any new information?

QUESTION: Yes.

DR. GOLD: I think from the point of view of the studies that I was referring to, one has enough examples on the front surface and one is very desirous of course of a very high definition for that purpose so that one would concentrate on looking at the best pictures on the front available at the present time.

It is naturally of great interest whether there is any consistent difference between the general type of surface that the front has as compared with the back, and if any photographs of this sort showed any consistent difference between front and back then of course one would have to think again, or indeed if it showed any features which are different in type from any features that occur on the front.

The definition of these pictures is not enough to make any case for a substantial general difference between front and back. On the whole it looks really rather a lot alike.

That is of course not to say that the pictures are not very valuable. But for the study of what shaped the surface into the way it now is, one is chiefly desirous of very high definition pictures. I regard these pictures as a first step in that direction, quite apart from being the first pictures of the back.

No doubt similar techniques will be used one day to get much higher definition pictures also than we can ever get from the surface of the earth. Such very high definition pictures will of course go a long way toward establishing which type of interpretation of the lunar features is correct.

QUESTION: Some commentators on that picture that the Russians took seemed to think that the back side of the moon was perhaps a little smoother than the front side of the moon,, where we had been lead to understand, some of us had at any rate, that if anything the front side of the moon ought to be smoother than the back because of the protection afforded to it by the earth.

DR. GOLD: The protection afforded by the earth is awfully little. It is quite negligible one way or another.

We wouldn't expect any big difference. On the other hand we have to say that on the front we have an eratic distribution of the mare which cover a large part of the front surface in a very haphazard way.

If one just considers the statistics of the six major mare areas, then of course it is very probable that the back will be substantially different. Six is not very good statistics. So you wouldn't expect that it would just have a similar amount of area of mare and highlands. In fact the suggestion is that there is more highland area and less mare area on the back.

QUESTION: The man at Fels Planetarium at Philadelphia suggested that.

QUESTION: Levitt suggested that.

DR. UREY: I read Levitt's book on that and I agree with it in a certain way and disagree in another way. I am sorry we do not have the other half of the moon here. Mare Imbrium was an enormous collision that occurred on that part of the moon. Then if you study the mare on the east part of the moon I think you must conclude that a great deal of the smooth area is intimately related to this collision in Mare Imbrium.

If one took Mare Imbrium off the front side of the moon, just this one collision, I think we would find that the front and back sides of the moon would not be terribly different.

And surely if we removed Mare Imbrium and the other big mare that you see there, Mare Serenitatis, I am quite sure that one would find that the two sides are very similar.

You see, if just two chance collisions with the surface of the moon make this difference, one would say this: I would expect the same collisions on the back as on this side to a first approximation, but I am not terribly surprised if they are not the same. In this case it is entirely understandable that the two hemispheres should be quite different in appearance.

Levitt said this. But he said it was very astronomically improbable that these collisions were on the back side. That is the only point I would disagree with. I would say that it was probable that they could be on the back side, but not surprising if they are eratically distributed.

QUESTION: He also said that these maria were produced by I think a very fluid lava which was picked up by the impact of these huge objects. Do you subscribe to that theory, Dr. Urey?

DR. UREY: This is an idea that has been current for a long time. I shall talk Friday night about the ideas of D. K. Gilbert, who discussed this subject two-thirds of a century ago. I can, in studying this, make out regions of the moon that look to me as though they might be due to lava flow. Then I can look at other regions and give you very good reasons why I think that lava flows did not occur at these places.

There are arguments both ways. I am very much inclined to the view that Dr. Gold has expressed, namely that much of the smooth area of the moon was once very finely divided material.

DR. GOLD: And I have argued about this for years and I think we are essentially in agreement with respect to somewhat different views about it.

He has felt that the erosion of the high areas of the moon were to a large degree responsible for the mare. I have thought that the broken up material possibly on the moon was largely produced by the big collisions that produced the maria.

That is, while recognizing that these things that Dr. Gold talks about are important in the formation of the moon, I think this other large effect was also of very great importance. And I think this is shown between the front and back sides.

If it were only a matter of slow erosion, one would expect the whole moon to look the same everywhere. But if it is due to big collisions, then one expects a difference. But as Dr. Gold emphasized in his papers when he published them some years ago, this smooth material, scattered through all of the craters at the south part of the moon, for example, where you will see the smooth areas, was this all due to a hot lava that oozed up inside all these craters and between them and gave this smooth material? As he said, it looks very unreasonable. In this way he weened me away from the idea that all these smooth areas are the result of lava.

But I think one must have to be objective about it and ask the question: What is the evidence in regard to the two. I am sufficiently doubtful about it that I would like to have samples of the moon. And as I often remarked, if we were completely sure what the moon is like before we get there, there is no use to go. You might just as well stay on earth and be happy with our ideas and not worry about the maria any more.

There are many things like this that need to be investigated and we have been discussing, yesterday, many things that might be done in space exploration that would tighten up our ideas about this and show us that indeed the material is one thing or the other, or a mixture of both, and to what extent it has these on the surface of the moon.

QUESTION: What do we know about the moon that we are sure of?

DR. UREY: There are many things known about the moon. As I often remarked about it, like all other scientific things we know, we have certain lines of evidence. If we wish to take any pleasure in the matter we must listen and understand the lines of evidence.

Is there such a thing as an electron? All we have are certain lines of evidence. If you will follow these lines of evidence you find that it is very convenient to suppose that there is an electron and we all talk about it as though we were completely a hundred percent sure that it exists, and I for one am.

DR. JASTROW: You interpreted the question in the semantics we would use. She meant what are we reasonably sure of.

DR. GOLD: We are reasonably sure of this: We see these pictures and we know the shapes of the craters, the heights of the rim, we know the darkness and lightness and color of the surface, we know that it is particularly colorless, remarkably colorless compared with terrestrial material, we know that it is generally rather dark.

QUESTION: What color is it?

DR. GOLD: Dark gray mostly.

DR. JASTROW: Black, essentially.

DR. GOLD: Dark gray or black.

DR. JASTROW: It is a very low reflectivity. That is the point. It reflects only ten per cent of the light on it. If you had a chunk of rock in front of you from the moon it would look black in this room, don't you agree?

DR. GOLD: Yes.

Mostly. The ray material looks lighter gray and the others darker gray.

DR. JASTROW: If you were standing inside one of those craters of course the bright parts would be fairly bright, like a beige, like your sweater more or less, but the dark parts would be pitch black. If you had artificial illumination, not as intense to the sun as a searchlight, the parts would show like dull coal. Quite dark.

DR. UREY: Another thing we know about the surface, for example, is that it is indeed covered with finely divided material, at least a very thin skin of finely divided material must be there in order to understand the temperature changes in the moon as it goes through its night and day. For example we know that.

What is there beneath this very thin layer on the surface? That we do not know.

QUESTION: How thin would that skin be?

DR. UREY: About two millimeters. About an eighth of an inch.

DR. GOLD: That is the minimum that has to be there. That is saying nothing about whether it is going on the same to a great depth.

DR. JASTROW: We also know -- and perhaps this is something you would be interested in -- we are almost sure it never had life on it. Now it is quite lifeless. We are quite sure in early times it had no life either because it is too small and light to hold an atmosphere, no life as we know it.

QUESTION: While we are on the business of life, yesterday Dr. John Strom reported that the instruments in the Malcolm Ross-Charles Moore balloon disclosed the existence of water vapor in the planet Venus.

DR. UREY: Very interesting indeed. It has been proposed by various people for some years that this might be the case.

I personally am not terribly surprised.

DR. JASTROW: You should be in the light of some of the things you have published.

DR. UREY: What do you say?

DR. JASTROW: Aren't you an advocate of the dry planet?

DR. UREY: You will find in the article I wrote, in the Handbuch der Physik, Volume 51, Menzel's and Whipple's idea, on the atmosphere of the planets, that I said that Menzel's and Whipple's idea of water, of oceans beneath the clouds, was perhaps the most probable explanation of the phenomena.

DR. GOLD: One had seen the top of the clouds on Venus. The question was whether the absence in the necessarily very imperfect means of distinguishing water vapor by looking through our own atmosphere, through the whole depth of it, was merely that the cloud top was sufficiently sharp so that there was very little water vapor above the top of the cloud, as indeed can be the case with terrestrial clouds.

The clouds could be water vapor, and the only thing that the previous measurements had indicated was that above them there was not very much water vapor. In this measurement, if correct now, would suggest that perhaps the clouds themselves are water vapor.

QUESTION: Then you would disagree, Dr. Urey, with Dr. Opik, who feels that there is no water at all on Venus.

DR. JASTROW: He hadn't read today's paper yet.

DR. UREY: If I had not been supplied with this information I would have said that the evidence is uncertain, that water is a possibility below the clouds is certainly a possibility, and so forth.

There is an interesting characteristic of the clouds of Venus, namely that they are colored just in the ultraviolet, just beyond the visible. I think this probably means that there is some dirty material in this atmosphere as one might expect in the absence of oxygen. If oxygen disappeared from the earth I think the atmosphere of the earth would immediately acquire a great many carbon compounds of the type we find in petroleum or that are given out by plants and things like that.

I rather think this indicates that that sort of thing is present in the atmosphere of Venus.

QUESTION: What does this do to the possibility or impossibility of life on Venus?

DR. UREY: I should say that without water it is not possible to expect life on a planet. I believe it could not evolve. All life as we know it lives in water, liquid water atmosphere. The fact that they have defected water in the high atmosphere of Venus would indicate that it is possible that there are oceans below the clouds, and if there is liquid water I myself would think that the odds were strongly in favor that life would exist beneath the clouds.

DR. GOLD: There is the other problem that the radio measurements of the surface temperature of Venus, radio measurements referring to below the cloud level, radio waves penetrating the cloud level can nevertheless measure the temperature, that those measurements come up with temperatures that are rather too high.

DR. UREY: That is right.

DR. GOLD: It could be that they are at fault.

DR. UREY: That is right.

DR. GOLD: But they would indicate temperatures of something like at any rate in excess of a hundred degrees centigrade, something like two hundred degrees centigrade which would be much too hot for any form of life as we know it.

QUESTION: Much too hot for liquid water also.

DR. GOLD: At our pressure. But of course we do not know the depth of the atmosphere of Venus.

DR. UREY: In addition to that I think the probability of life living at such temperatures is rather low and the reason for it is this: Life consists of some very mild chemical reactions that take place in a very complicated way.

If you raise the temperatures to two hundred degrees, chemical reactions involving the element carbon all become very rapid and very destructive of carbon compounds.

So that I think that this complicated set of chemical reactions of great complexity would be most difficult to maintain. I think for this reason I would be doubtful of life if the temperatures are so high as Dr. Gold mentioned.

MR. ROSEN: Are we agreeable that 530 might be a breaking point?

QUESTION: Could I ask a question back on the Russian moon pictures? From what you could see you indicated that they weren't clear enough to see much, or to give you much information. But from what you could see did it have the same type of craters?

DR. GOLD: Let me first make quite clear that I greatly admired their feat, and I am interested in what they show. It is only the specific question can I show more about the derivation of the surface.

QUESTION: The question is, from what you could see, from what evidence there was, did you have the general impression that there were craters such as this?

DR. GOLD: Oh, yes.

QUESTION: Was that at least clear enough?

DR. GOLD: Yes. It looks in principle very much like this if you took this kind of picture for definition, amplitude, and intensity graduation on it. Rather fewer seas. but that is as fully expected. Rather fewer seas and a high proportion of highlands but with much of the highlands, highland crater structure probably looking much like this, but not sufficiently well resolved in intensity to show the slight graduation of intensity needed in order to discover the presence of the craters.

There are plenty of craters visible. I have actually seen the pictures better than those which appeared in the press, some which were flown over from Moscow.

They show perhaps in one respect a feature which at first glance might have been thought of as different from anything that the front has, namely they show one bright fairly straight long thing. That thing has been interpreted in the first quick interpretation by the Russians as a mountain range.

And if it were as long and straight a mountain range, then it would be quite different from anything that is on the front of the moon. Indeed it would be very suggestive of internal origin, whereas on the front we see nothing that is of internal upheaval origin.

On the other hand, I now feel that I have seen what I believe to be the best composite picture that the Russians have devised out of the several pictures that they took, and on that I feel if I look at that that I am just as entitled to make by judgment as to whether that is a mountain range as anybody else.

To me it does not look like a mountain range. It looks much more like a very strong ray of the same nature as the rays from Tico, but a very bold bright one drawn for a major part across but fairly close to the edge of one of the seas on the back.

QUESTION: I have one other point. At the time that we were attempting our first moon shot in August of 1958, there was some talk about the danger of contaminating the moon from the point of view of further exploration.

DR. JASTROW: Biologic?

QUESTION: Biologic contamination. Did you people discuss this at all, and how do you feel about it? Is it important or trivial?

DR. JASTROW: It is not trivial. It has been discussed extensively. The consenus of informed biological and physical opinion is I think that this danger is most remote on the moon, which is surely a very sterile, lifeless object. For the planets, that is another story. That is a question still to be considered.

QUESTION: Dr. Jastrow, you mentioned that there were several discussions and indeed arguments during the conference. Could you give us an example of that?

DR. JASTROW: Let me finish this first. Still, to be on the safe side, we are taking precautions against contamination.

QUESTION: I have another question about the Russian pictures before we get off.

How many pictures have people in this country seen? The press has only seen one, and Life had two.

DR. JASTROW: I saw several at the Rocket Society Meeting. That is the best that I can tell you.

QUESTION: I got two from the Russian Embassy.

DR. JASTROW: They pooled them all together.

QUESTION: Are they all essentially the same?

DR. JASTROW: I can't resist commenting. Harrison Brown wears two hats. He is interested in the moon. He also has a prominent position in oceanographic research in the academy. I can't help but ask him whether he is interested in the back side of the moon as well as the oceans.

DR. BROWN: They are both important.

QUESTION: I would like to ask how much doubt there is on these radio determinations of the temperature of Venus. Dr. Urey said if it is as hot as it seems to be, life is improbable.

DR. UREY: One of the things that one must recognize about a scientific subject that is under development is that one can only suspend judgment about certain things at certain times. If we take the measurements as they seem to be, the surface of Venus appears to be hot. But I believe even the authors of this work, as well as all of us, who read about it, are a little reserved about accepting it as final. And we are hoping that more measurements will be made and more attempts to understand this will be made which may lead to some other interpretation. So it is simply a suspended judgment.

DR. JASTROW: Dr. Urey is the originator, by the way -- I think it is his original idea -- of an argument which indicates that Venus is either entirely arid, entirely dry, or entirely wet. I thought he favored the dry planet theory. But the presence of water vapor in the atmosphere, combined with Harold's argument, suggests that Venus may be one vast ocean teeming with countless monsters, because the conditions are favorable except for this in any case isolated observation on the radio temperature.

QUESTION: Why do you necessarily want to go all out one way or another?

DR. UREY: Let me put it this way. Carbon dioxide reacts with the rocks on the Earth to produce limestone and sand. This reaction is taking place all the time. From our chemical thermodynamics we can calculate what the equilibrium pressure of carbon dioxide in the atmosphere would be, how much will be absorbed before it stops.

The calculations are not very accurate. But they indicate that the pressure of carbon dioxide could be considerably less in the present atmosphere of the Earth than is observed.

This occurs on the Earth because water runs down the mountain sides. It contains carbon dioxide and the carbon dioxide has an excellent chance to react with the rocky materials and produce calcium carbónate and silicate dioxide -- limestone and sand.

Suppose it were completely dry. Then the reaction couldn't take rapidly at all. It would proceed in the surface of the dry rocks to a slight extent and then all stop. There is another way you can stop it. You can cover the whole planet with water and again there is no way for the carbon dioxide to get into the solid materials and react effectively.

There is another explanation of this. It may be that the carbon dioxide has already reacted with all the surface rocks of Venus and there is still a supply of carbon dioxide left over, with nothing to react with. So you can meet this in a variety of ways. I originally said it must be dry.

Then Menzel and Whipple came along and said it could be completely covered with ocean, and I agreed. These are other possibilities.

DR. JASTROW: The background is that there is a huge amount of carbon dioxide in the atmosphere of Venus. It is the one thing we have discovered up to this time. Harold's argument is based on that observation.

DR. BROWN: It should be pointed out also that an atmosphere composed entirely of carbon dioxide would behave entirely differently from our own atmosphere. Carbon dioxide radiates quite efficiently in the infra red, so you have really a very complicated problem. You have visible light coming in, becoming degraded, getting trapped in there, sort of in a greenhouse. That in itself would make it hot. But the extent to which the carbon dioxide creates a particular kind of circulation pattern in the Venusian atmosphere will determine how rapidly it can cool off.

QUESTION: Can we, by observing Venus, do any kind of radiation balance in that sense?

DR. UREY: My colleague at the University of California was so thoughtless as to be at Los Angeles instead of La Jolla. Dr. Mintz has been making calculations on that very subject. He will purlish a paper before long. There are immense calculations that have to be made. We do not know enough about it to fix the fundamental conditions reliably. Such attempts are being made.

DR. BROWN: One more thing should be pointed out about the Venusian atmosphere. This is very tempting and perhaps suggestive.

If we were to take all the calcium carbonate on the Earth in the form of limestone and heat it up, and put all the carbon dioxide into the atmosphere, we would have something like ten atmospheres of carbon dioxide. It would be very similar to the Venusian atmosphere. This leads one to suspect that perhaps there are no major deposits of limestone to be found on Venus. If that is true, then it would mean that although there might be life there, there might not be life with bones and supporting structures.

QUESTION: Jellyfish?

DR. BROWN: Jellyfish.

QUESTION: Could these high temperature indications from radio telescope observations be due to synchrotron effects similar to that on Jupiter?

DR. JASTROW: I think not. With the violent storms and so forth that occur on Jupiter, Venus is not a source of radio noise. I think that is the answer.

QUESTION: If you were to stand off from Earth and try to get measurements of the Earth's temperature, what effect would you say the ionosphere would have on your measurements? Could that be possible?

DR. JASTROW: I don't think the ionosphere would affect you very much. It would depend on the wavelength you would use. I think this is the answer to the question of the radio measurements of Venus' temperature. I believe one has to reserve judgment on the interpretation of those, the surface temperatures. But it is interesting.

QUESTION: In other words, you don't know how deep in the atmosphere those measurements relate?

DR. JASTROW: It is possible that that contributes to the uncertainty. I think one should reserve judgment.

QUESTION: What are some of the temperatures that were deduced for Venus before radio telescopes made theirs?

DR. JASTROW: They vary all over the lot, depending on the wavelength. Is that right, Harold? There is a considerable variation, a couple of hundred degrees.

DR. UREY: Yes.

QUESTION: From what to what?

DR. JASTROW: I can't remember. They vary by several hundred degrees.

DR. UREY: The temperature of Venus, as determined from infra red measurements at Mount Wilson, ran, as I recall, about 235 degrees absolute. That would be minus forty degrees Centigrade. And that is about minus forty degrees Fahrenheit. This was the observed temperature from the infra red.

In the second place, Dr. Chamberlin and Dr. Kuiper McDonald studied certain bands of carbon dioxide in the atmosphere and they got somewhat above the freezing point of water, about ten degrees Centigrade. If the measurements are correct, they are measuring different layers in the atmosphere. Do you see?

Then if you are going to understand it, we must try to construct an atmosphere that has different levels of temperature which can be accounted for on the basis of the observations and theory. This has not been done satisfactorily at all.

MR. ROSEN: It is now 5:31, gentlemen.

DR. UREY: We are having a good time. We are just born teachers.

MR. ROSEN: If you have no objections to staying on, I leave it up to the press.

DR. JASTROW: I think we can do it informally.

QUESTION: Milt Rosen -- not Herb Rosen -- put forth the theory a short time ago that he felt that an early manned lunar probe was advisable. From what you gentleman have said here about the things to be learned, this would seem to support this theory. Would you comment on that?

MR. ROSEN: As long as we avoid dates.

DR. UREY: I don't know any dates.

DR. JASTROW: We don't know any dates and we should comment that a great deal can be learned in the natural course of events with regard to the difficulties of bringing a man to the moon, and with regard to the fact that he must be brought back. It is certain that much of what we wish to know will be learned by remote control through unmanned instrumentation before we have a planned landing.

QUESTION: Dr. Urey, you said a while ago something which shocked me. I guess you didn't mean it. You said if we knew the answers to all these questions we could sit home and forget about it.

DR. UREY: Surely.

QUESTION: I was under the impression that there was some objective in reaching the moon, other than simply learning about it.

DR. UREY: I myself am immensely thrilled by a feat of exploration. I was thrilled when somebody got to the South Pole and the North Pole. I would like to see somebody climb Mt. Everest. I was immensely thrilled by the flight of the instruments of the USSR to the back side of the moon, sending us an approximate picture of it. There are many things in life besides science. I enjoy all of these things.

QUESTION: Then to go a little further than that, isn't there a real objective in getting to the moon, aside from finding out if there is life on the moon?

DR. UREY: Not from the scientific point of view at all. Let me put it this way: Scientists, by and large, are interested in understanding the universe; Not in doing practical things.

DR. JASTROW: I wouldn't go so far as --

DR. UREY: Let me finish the statement. We are interested in understanding. As we pursued our scientific work we found that the side products of our scientific work turned out to be immensely useful. I have an example of that.

Discovering heavy hydrogen some 25 to 30 years ago, I thought it would never be useful at all. One of the biggest chemical plants in the United States makes heavy water. It may turn out to be the most important fuel that man has ever discovered. I am very much pleased that this is the case. But it wasn't my primary interest.

And so when we are talking about this we are really not wondering about whether we are going to put colonies on the moon. We are trying to understand the moon. If that it turns out to be a good place for summer resorts, it pleases us very much and we may go along and pay it a visit some day. This is what I mean by it.

Our primary concern as scientists is to try to understand this universe. I am much more interested in the origin of the solar system, the origin of the elements, and the past history of the moon, and from it the past history of the Earth and what the planets may be like, and these things, than I am in making a trip somewhere. That is what I want to say.

DR. JASTROW: We have some specific objectives related to the origin of the solar system. They are based on what we know at this time. But really the exploration of the moon is something akin to Columbus's setting forth from Spain. He thought he was looking for India. He found something quite different. Harold has said in as beautiful a way as I have ever heard it, that we don't know the importance of the things we will find. We can't possibly know.

QUESTION: I would like to ask Dr. Brown if he would rather dig a mole hole than send all these instruments to the moon?

DR. BROWN: Which would I rather do?

QUESTION: Yes.

DR. BROWN: I would rather go to the moon any day.

QUESTION: Doctor, you sounded a little jealous.

DR. UREY: If I may make a remark on that, there is no reason to make a decision as to which we do.

DR. BROWN: No. What we individually would rather do.

QUESTION: Dr. Jastrow, in view of what happened last Thursday, I wonder if there is any particular significance to the fact that the subject of lunar exploration has been put in the hands of the Theoretical Division?

DR. JASTROW: No, it hasn't been. We are interested in lunar research because of its importance in cosmology and its general importance, and we arranged this conference some eight or ten weeks ago. I actually do not know enough about our schedules to have been aware that it followed so closely on the shot that was attempted. We intend to continue our interest in this area. It is one of several branches that we as theorists are interested in. The others are astrophysics, planetary properties, geophysics, upper atmosphere physics, et cetera.

QUESTION: Will we also have symposia and press conferences on those subjects when they take place?

DR. JASTROW: That is up to Mr. Rosen and you, if you people are interested in this sort of thing.

MR. ROSEN: Are you?

QUESTION: Yes, I would be.

MR. ROSEN: We will see if it can be arranged.

(Thereupon, at 5:36 p.m., the press conference was concluded.)

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NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

NASA RELEASE NO. 59-261
DU 2-6325

December 2, 1959

NOTE TO EDITORS:

Attached is a release describing NASA's Project Echo -- an experiment designed to launch into orbit early next spring a 100-foot inflatable sphere for use as a passive communications satellite.

This release is an exception to a long-standing policy of not publicly announcing space experiments until they have been performed -- whether successfully or not.

Scientists in the communications field who could make constructive use of experiments involving the sphere must have notice sufficiently in advance to make necessary technical preparations.

Therefore, we are making this exception because of the intense interest expressed by the scientific community for volunteer participation by others than those who will participate on a contractual basis.

The news media, in addition to the normal technical channels, can help us inform the scientific communities here and abroad of some of the pertinent details of this experiment.

Walter T. Bonney
Director
Office of Public Information

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

FACT SHEET

December 4, 1959

For Release: When capsule on board recovery ship

LITTLE JOE CAPSULE RECOVERED

The Little Joe capsule launched at [redacted] today by the National Aeronautics and Space Administration from Wallops Station, Virginia, was recovered at [redacted] by the USS [redacted].

Scientists will analyze data recorded on-board to determine the operation of the Project Mercury pilot escape mechanism at high altitude.

The rhesus monkey carried on board to obtain measurements of the biological response to space flight has been removed from the capsule and survived the flight. It will be taken to the School of Aviation Medicine at Brooks AFB, Texas, for post-flight analysis.

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

NASA RELEASE NO. 59-261
DU 2-6325

For Release:
Monday, a.m.
December 7, 1959

NASA TO LAUNCH 100-FOOT SPHERE

Plans to launch a 100-foot-diameter inflatable sphere into orbit as a communications experiment were announced today by the National Aeronautics and Space Administration.

Known as Project Echo, the experiment calls for the launch of an aluminum-coated sphere into a 1000-mile-altitude earth orbit early in the spring of 1960. The launch is to be in a northeasterly direction from Atlantic Missile Range with an inclination of about 50 degrees to the equator.

The objective of the experiment is to test the feasibility of a passive reflector communications system on a global basis. The vapor-deposited aluminum skin of the big satellite will give it a high degree of radio wave reflectivity. After the 100-foot sphere is placed into orbit, NASA hopes to establish two-way radio communications between the East and West Coasts of the United States by bouncing signals off the sphere.

Once in orbit, the satellite will pass over all countries located between 50 degrees north latitude and 50 degrees south latitude. Its orbital period will be about 120 minutes. It is expected to pass over every portion of the United States except

Alaska, enabling many independent researchers in the communications field to make use of the large sphere for propagation experiments. The maximum time of mutual visibility between the East and West Coasts for any one pass will be about 16 minutes.

The launch next spring will be the first of three Project Echo satellites. Each will be a 100-foot reflecting sphere developed at NASA's Langley Research Center, Virginia. The satellite will be an inflatable structure of .0005-inch-thick Mylar plastic coated with vapor-deposited aluminum to provide radio wave reflectivity of at least 98 percent up to frequencies of 4,000 megacycles per second. The sphere will weigh about 150 pounds. It will be as bright as a zero magnitude star -- about as bright as Vega.

The payload package will be carried into orbit by a Delta launch vehicle.

At launch, the sphere is folded inside a 28-inch diameter container. About four pounds of water in a plastic bag is carried inside the sphere. At injection, the payload container opens by explosive bolts and the sphere begins inflation with the residual air inside the satellite. Inflation is completed as water in the plastic bag turns to vapor and expands.

The two major facilities involved in the experiment will be NASA/Jet Propulsion Laboratory's Goldstone Tracking station in California and a Bell Telephone Laboratory station at Holmdel, New Jersey.

Goldstone will transmit a 2390 mc/s signal for interception at Holmdel. The Bell antenna will transmit a 960 mc/s signal to be picked up by Goldstone.

Project Echo is NASA's first step in a long range investigation of the application of artificial earth satellites to global communications. Such satellites may one day be used as relay stations for transmission of signals, voice, and television.

END

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

For Release:
Monday, p.m.'s
December 7, 1959

Statement by T. Keith Glennan

Administrator

National Aeronautics and Space Administration

before the

Institute of World Affairs

Pasadena, Calif., December 7, 1959

Mr. Chairman, Distinguished Guests, Members and Friends of the World Affairs Council:

It is an honor to speak from this platform tonight. I am particularly grateful for the opportunity to bring to the members of this distinguished audience a brief discussion of our national space program. As citizens, you should be aware of the problems and promises that challenge the nation in the field of space exploration. As members and friends of the World Affairs Council, you will be interested, I am sure, in the possibilities for useful and effective international cooperation that reside in this new area of scientific activity.

As one of my colleagues has put it--"When one considers the vast distances of the solar system --- 93 million miles to the sun; 26 million miles to Venus, the nearest planet; 3,680 million miles to Pluto --- and when one catalogs the problems to be solved and the new knowledge that is needed in almost every branch of science and technology from magnetohydrodynamics to cosmology, from materials to biology and psychology, the magnitude of the task before us becomes apparent. It is a task that challenges the peoples of the earth as a whole. There is room for cooperation of men of many skills and of nations, large and small".

In this context of viewing space research as an instrument for the development of meaningful cooperation between nations, let me first describe the program of the United States. I will then tell you what I know of the program of the Soviet Union. Finally, I shall discuss the manner in which international cooperation is beginning to develop. In doing this, I shall borrow liberally from reports and papers presented at international meetings which have been held in the last several months.

The interest of man in outer space began long ago among uncivilized peoples to whom the face of the sky was clock and almanac; the celestial bodies, objects of worship. Exploration was at first by visual observation, later aided by armillary spheres and quadrants, and still later by more precise measuring instruments, telescopes, and spectroscopes. The information obtained was that borne by the light that was transmitted from the distant celestial object through the atmosphere to the observing instrument on the ground. In recent years the light waves have been supplemented by radio waves as carriers of information from the stars and planets.

Men of many nations have contributed through the centuries to the exploration of space by the methods of astronomy. The history of advances in astronomical knowledge and technique includes the records of Chinese, Babylonians, Greeks, Arabians, and of nearly every nation of the modern world. International cooperation was early recognized as essential and beneficial; the countless number of the stars and the vastness of space present mankind with a truly global task.

The picture of the universe obtained by the astronomers early stirred the imagination of men to speculate about the existence of life elsewhere in the universe, about means of communication with distant stars, and in the last centuries about the possibility of the travel of man to the moon and planets. Some sought to apply the science and engineering of their day to describe the vehicles to be used. For example, Jules Verne published in 1865 in "From the Earth to the Moon" a description of a gun-launched projectile carrying passengers to orbit the moon. Today we have taken the first steps to bring this inspired vision to reality. The exploration of space by unmanned vehicles carrying scientific apparatus began on October 4, 1957; exploration by man will follow in due course.

Now that date--October 4, 1957--did something more than mark the successful launching of a satellite into an orbit around the earth by the Soviet Union. It brought this nation to its feet in a sort of bewilderment. How had this come about? Our leadership in science and technology, our genius for applying new knowledge gained through research to the solution of the problems of mankind--these were being challenged--and in a most dramatic way. Initial reactions of skepticism began to give way to a sober realization that space research was more than a scientific activity. In the hands of a determined and able competitor, it was a mighty instrument for propaganda and a symbol of international prestige.

In mid-1958, the National Aeronautics and Space Act was signed into law and the National Aeronautics and Space Administration was established. The Act begins with a declaration of policy and purpose which reads thusly:

> "The Congress hereby declares that it is the policy of the United States that activities in space should be devoted to peaceful purposes for the benefit of all mankind". It also declares that "The aeronautical and space activities of the United States shall be conducted so as to contribute materially to (among other objectives) cooperation by the United States with other nations and groups of nations in work done pursuant to this Act and in the peaceful application of the results thereof."

I think I will not take the time tonight to describe the growth of NASA to you. We do have in operation several large research centers, three of which are located in California. One of these is well known to this audience--the Jet Propulsion Laboratory operated by Cal Tech under contract to NASA. The Congress being willing, we will add to our research center roster in mid-March the Huntsville, Alabama group under the direction of Dr. Wernher von Braun.

By June 30, 1960, we will employ more than 15,000 people in the government-operated centers alone. The Jet Propulsion Laboratory complement of able people adds another 2400 to that total. Our budget last year totalled 335 million and this year the Congress appropriated 501 million for our use. We are in the middle of budgetary discussions for the 1961 fiscal year and I can say only that our resources for the next fiscal year will be larger by a significant amount.

Now as to our program--and here I must compress a two hour discussion into a five minute summary--let me say that it includes research in most of the areas of the physical sciences and in certain of the areas of the life sciences. One of the principal objectives of current space activity is the study of the space environment by the undertaking of scientific experiments using sounding rockets, man-made earth satellites, man-made planets and deep space probes. In the United States, we have used the term "space science" and a shorthand expression for experiments in physics, chemistry, bio-science, astronomy, astro-physics and geophysics. All of these space science experiments will employ instruments transported into the upper atmosphere and outer space.

The NASA objectives include the investigation of the uses of earth satellites to perform more efficiently and effectively some tasks which are now carried out by the other means and to perform other tasks which cannot be done at all with present means. The applications which seem most promising at present are those directed toward weather observation, analysis, and forecasting on a global scale; the improvement of long distance radio communication; the study of the size and shape of the earth and of the distribution of land masses and water; and all-weather global navigation. It is believed that such applications brought to successful fruition will improve the well being of mankind everywhere.

NASA program objectives, presumably like those of other countries, include, too, the orderly development of means for the manned exploration of space. En route to the long-range objective of manned exploration of the solar system are the temporary ballistic flights of man into space and return (already accomplished with animals), manned flight for one or a few circuits in the simplest vehicle in an orbit well below the level of the Great Radiation Belt, manned flight in advanced maneuverable vehicles, in larger satellites carrying several men, in permanent manned orbiting space laboratories, manned flight to the vicinity of the moon and return to earth, and manned landing on the moon and return.

NASA's present project in this field, Project Mercury, has been repeatedly described in the international public and technical press. Its successful completion requires the cooperation of several countries in permitting the installation and assisting in the operation of portable tracking radars, communication stations, and telemetry receiving stations at suitable points along the intended course. Negotiations currently under way promise that this cooperation will be forthcoming generously.

Even the first steps in the manned exploration of space are very expensive as may be inferred from the presently estimated cost of Project Mercury of $250 million or more. The resources required for the advanced missions I have mentioned may well demand a world-wide collaboration. Thus this activity may serve to give a true measure of man's response to the challenge to discover and explore the new frontier of our day.

In order that the programs just discussed can be carried out at an ever-increasing level of complexity and scientific significance, it has been obvious that launching vehicles and space propulsion systems must be provided. An early task of NASA, then, was the planning of a program of rocket and vehicle development in cooperation with the Department of Defense. Such a program must provide for the flying of all the desired missions with a minimum number of new rockets and new vehicles. As in other countries, our present launching vehicles are assembled from rockets developed in the ballistic missile program and available smaller rockets. For the increased thrust that we so much require for future mission, two new developments have been started in the United States. The first of these is being developed by Dr. von Braun and his people--the Saturn vehicle--a cluster of eight existing rocket engines to give a capability of about one and one-quarter million pounds of thrust. The second is a single chamber rocket engine of one and one-half million pounds thrust under development by the Rocketdyne Division of North American Aviation. It is expected that this engine can be clustered to give six million pounds thrust or more.

In addition to these first stage booster rockets, several upper stage rockets are under development including some using high energy fuels. In addition, nuclear rockets are under development by the AEC and NASA along with the general application of nuclear energy for auxiliary power in space vehicles.

Of particular interest to other countries may be the launching vehicle system under development by NASA and known as the Scout. This is a four-stage, solid-propellant satellite launching vehicle that will carry 150 to 200 pounds into an orbit 300 miles above the earth's surface. It will be more economical than existing vehicles; hopefully it will cost no more than $600,000 per firing. We expect to use this vehicle, if its development is a success, in early international cooperative programs.

Now there is no point in launching a satellite or an experiment toward the moon or the planets if we have no means of tracking the space experiment and acquiring from it the information collected by the various sensors carried aloft. Thus we have had to build a network of tracking and data acquisition stations that today covers most of the globe. Fortunately, we inherited some stations from the activities carried on under the International Geophysical

Year program and thus were able to launch a good many useful experiments during the past year without waiting for the construction of the stations necessary to complete the network.

Now, what of the Russian program? I suspect that most of you know more about it than you do of our own. From information given us by a variety of sources - some of them Russian, - it appears that they have assigned their top scientists and engineers to this new field. They possess rockets that are estimated to be twice as powerful as our largest--the Atlas intercontinental ballistic missile. They have launched three successful satellites and three deep space probes. One of these now orbits the sun, another landed on the moon and the third photographed the far side of the moon as it went into an orbit that initially linked the moon and the earth. Nothing has been said by the Russians about their failures whereas our failures, as well as our successes, are prominently displayed for all the world to see. But it does seem that their space vehicle system is highly reliable, suggesting that they have fired it much more frequently than any of the variety of systems we have been forced to use thus far.

As to scientific results to date, it is the opinion of knowledgeable scientists that we have done as well or better than the Russians. They have been able to couple spectacular technological accomplishments with useful scientific experiments whereas our more modest technological efforts--because of our lack of reliable launching vehicles of high thrust--have turned up really significant amounts of new and important scientific information.

More important to the Soviet Union than their scientific achievements, however, has been the fact that they have been successful in making their spectacular space accomplishments appear to many nations as a valid measure of their sophistication in all branches of science and technology. More recently, they have been active and successful in creating the impression that their achievements in space research and exploration are a valid measure of the strength of their communist system as compared to our democratic way of life. All in all, the Soviet Union has made and is making hay while the sun shines on their satellites and lunar probes.

Now let me turn to the matter of international cooperation as we see it today. You will recall my reading that section of the Space Act governing our activities that encourages us to develop programs of international cooperation. An Office of International Programs was established by NASA in November 1958. Exploratory talks were conducted with the scientists of other nations and a pattern for cooperation was established with the blessing of the scientific community. We are now quite completely occupied with discussions with a dozen groups from as many countries interested in associating themselves with the United States program.

It might be well for me to describe to you some of the activities which may form the basis for international cooperation and which arise from the global nature of research in space. The desirable types of activity, it seems to me, are exchanges of scientific and technical information and data, exchanges of scientists, coordinated programs of observation and experimentation, and cooperative programs of space exploration.

Exchange of information in its usual form consists of the exchange of publications and the holding of international scientific meetings. In the space activities initiated during the IGY it was found desirable to exchange information on the planning of experiments, to give prompt notice of launchings, early information on orbits, and such other data as would permit participation of others in observations of scientific value. It is the desire of the United States to progress toward the complete re-establishment of these procedures.

It has been remarked earlier that space science is not a new scientific discipline but comprises the use of new tools of experimentation by trained scientists in physics, geophysics, astronomy and similar established fields. The exchange of scientists between countries permits a more rapid transfer of the new techniques than can be accomplished by publications or presentation of papers. NASA has established a few fellowships available to scientists of other countries and has provided research opportunities to a few guest scientists. Exchange of scientists in addition to providing training in new techniques may also be used for substantive participation of senior scientists in cooperative programs.

It is obviously desirable that national programs in the space field be coordinated to avoid undesired duplication and to provide the enhanced increase in knowledge that comes from coordinated efforts. This coordination was well done under the non-governmental international committee for the IGY (CSAGI) and we look forward to the early establishment on a more permanent basis of the Committee on Space Research to continue coordination of basic scientific research in the space field. There is need for coordination in program planning, and in the execution of certain programs. Activities in the tracking of satellites and in the reception of telemetered data, in research on the upper atmosphere and ionosphere by means of sounding rockets launched simultaneously in various parts of the world, in investigation of the ionosphere by observation of radio signal from satellities, and in laboratory and theoretical research in areas supporting space activities are examples of program areas in which international coordination would be most productive.

The ultimate step in international cooperation is joint participation in a single program with participation of scientists of two or more countries in the design of experiments and in the preparation of payloads for rockets, satellites, and space probes. As I have said, discussions are under eay between NASA scientists and their colleagues from other countries with the view of beginning activities of this type.

As a matter of fact, the international character of cooperative space activities in which we are engaged is already broad. Our radio and optical tracking network is composed of stations located in, and often operated by scientists and technicians of, Argentina, Australia, Chile, Ecuador, India, Iran, Peru, Spain, and South Africa. Other cooperating stations are situated in England, West Germany, and Japan. The new Project Mercury tracking stations will expand this list to include additional countries to the South and in Africa, along the planned orbit of the manned capsule.

Beyond this, tentative arrangements for substantial programs of joint exploration of our spatial environment have already been made with the United Kingdom and Canada. Additional cooperative programs have been proposed by a number of Pacific and European national space committees. These are substantive proposals, in which each nation will make its own scientific and technical contribution in a truly joint effort toward mutually agreed objectives. The preparation and execution of these programs will not be accomplished in a few weeks, or even months, but the achievement of their objectives, with the attendant scientific interchange, will enrich all.

As an evidence of our interest in international cooperation, we would be most happy to offer the services of our tracking network in support of the scientists of the Soviet Union when and if that nation undertakes a manned space flight program. Data could be acquired and transmitted in its raw state to the Academy of Sciences in Moscow. A precedent for this sort of thing has been established in the IGY operation when the United States supplied, to the Soviet scientists, as of July 1959, some 46 tape recordings of Sputnik I, II and III. Should special recording or data read-out equipment be required, I am sure that we would be happy to provide them or to utilize equipment furnished by the Soviet scientists. In such a cooperative venture we could help them to keep in continuous or essentially continuous contact with their astronaut.

Ladies and gentlemen, it has been my lot to be associated with exciting new ventures throughout most of my life. As I look back over the years, it seems that I have been happiest and have worked most diligently when the activity in which I was engaged had a vital role to play in the affairs of men. Thus my association with the Atomic Energy Commission was important to me because I was convinced, early in that association, that our nuclear weapons strength was the one most powerful deterrent to the initiation of a shooting war by another great power. Now I find myself in this exciting, difficult and important field of space research. To me, one of its greatest appeals is the opportunity it offers for the development of a sound program of international cooperation in the science and technology necessary to the exploration of outer space.

After all, science is truly an international language. And space is an all pervasive arena with plenty of challenge for anyone who possesses the curiosity and energy to attempt the solution of its mysteries.

"To explore space to gain knowledge of the physical universe in which man lives; to explore space as a demonstration of his mastery of advanced technology; to open space to his own travel to satisfy his desire to see and experience for himself; to explore applications of space technology to improve world-wide communications and weather forecasting -- all of these aims reflect as in a mirror the desires of men everywhere."

Out of the efforts of the dedicated and inspired men of all nations may yet come that common understanding and mutual trust that will break the lock step of suspicion and distrust that divides the world into separate camps today. Whatever the outcome, we cannot fail to make the effort.

Thank you.

NASA Release No. 59-264

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

NASA RELEASE NO. 59-270
DU. 2-6325

For Immediate Release
December 8, 1959

OSTRANDER NAMED TO HEAD NEW NASA HEADQUARTERS UNIT

The National Aeronautics and Space Administration is setting up a new headquarters unit for rocket vehicle development. It will be headed by Air Force Maj. Gen. Don R. Ostrander, now deputy director of the Advanced Research Projects Agency.

Gen. Ostrander will join NASA on Air Force assignment about January 1 as Director of Launch Vehicle Programs.

"We are establishing the rocket development group because of our expanding programs," said NASA Administrator T. Keith Glennan. "For this job, we are particularly pleased to be able to call on the special talents of Don Ostrander."

The new post creates a fourth major unit in the NASA headquarters organization. Other principal offices are Business Administration, Aeronautical and Space Research, and Space Flight Development. The new division will mean some reorganization within the latter two groups.

The Space Flight Development team, headed by Dr. Abe Silverstein, will have responsibility for space craft design and construction, mission planning and in-flight research and operations.

The Aeronautical and Space Research group, directed by Ira H. Abbott, has charge of advanced research in aeronautical and space areas.

Gen. Ostrander's group will be in charge of NASA booster development and rocket launching operations, including the Saturn team of the Development Operations Division, Army Ballistics Missile Agency, at Huntsville, Ala., on implementation of its transfer to NASA.

Gen. Ostrander, who is 45, has served as acting director of ARPA since October. A 1937 graduate of the United States Military Academy, he has had a part in the direction of a number of Air Force missile programs over the past 15 years.

He moved into the ARPA post after more than a year's duty as assistant to the NATO assistant secretary general for guided missile production in Paris. Before that, he held a variety of administrative posts within the Air Force Air Research and Development Command going back to 1951, working on weapons systems and missile programs.

He was promoted to brigadier general in October, 1954, and got his second star in March, 1958.

During World War II, he served as ordnance and armament officer with the Eighth Air Force Interceptor Command in England. In this role, he helped modify the P-38 airplane to accommodate a Norden bombsight and bombadier. His decorations include the Legion of Merit with cluster.

He was born in Stockbridge, Mich., attended local schools and studied engineering at Western State College, Mich., before entering West Point.

Mrs. Ostrander is the former Frances Ann Dunn of El Paso, Texas. They live in Alexandria, Va., and have three children: Mary Frances, 10; Don, 9, and Sally Ann, 7.

- END -

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

For Release: Upon Delivery
About 1:00 p.m., Dec. 10, 1959

WHERE ARE WE GOING IN SPACE?

by
John A. Johnson
General Counsel, National Aeronautics and Space Administration,
to the National Security Industrial Association
Mayflower Hotel, Washington, D. C.
December 10, 1959

It is a real privilege, as well as an honor, to talk to an audience such as this about our Government's program of space exploration. I doubt that it is possible to find a group anywhere that is more broadly representative of all elements of American industry contributing to this program than the National Security Industrial Association.

We at NASA have a deep conviction that everything we are doing serves the security interests of our nation. So I naturally hope that you construe the term "security" in the title of your organization broadly enough to include the mission of our agency. In fact, during the past year, your Association has left no doubt about its interest in one of our problems with which I am personally very much concerned -- NASA's patent policies.

As you know, our basic law, the National Aeronautics and Space Act of 1958, contains certain provisions making it necessary for NASA to deal with contractors in patent matters on an essentially different basis from the Department of Defense. The development of policies and procedures to carry out these provisions in the law has required the breaking of much new ground in a very complex field. The assistance which we have received from industry and the patent bar has been invaluable to us during this period. Members of your Association have contributed greatly with advice which we have felt to be generally fair and objective on a subject which, a year ago, had caused quite a few emotional outbursts.

We have recently proposed to the Congress that this portion of our law be replaced by a new section giving NASA broad authority to develop patent provisions for use in its contracts which will both serve the public interest and protect the equities of contractors. Just last Saturday a subcommittee of the House Committee on Science and Astronautics completed a week of hearings on our patent legislation proposals at which a large number of nongovernment witnesses testified.

But to get back to this matter of security, which is the prime concern of your Association. The evidence is abundant that both the Congress and the President regard

NASA and its mission as being important to the national security.

The National Aeronautics and Space Act of 1958 itself emphasizes this fact. The Act, which established NASA, contains two important declarations of national policy at its very beginning: first, "that it is the policy of the United States that activities in space should be devoted to peaceful purposes for the benefit of all mankind," and second, "that the general welfare and <u>security</u> of the United States require that adequate provision be made for aeronautical and space activities."

The field of procurement law provides further evidence of Congress' recognition that NASA's status is very much like that of the Department of Defense. The Act that established NASA also amended the Armed Services Procurement Act to make it specifically applicable to NASA. In fact, it applies only to the Department of Defense, the military departments, the Coast Guard, and NASA. Since NASA is subject to the provisions of that law just as are the military services, there are few substantial differences between the legal procurement authority of NASA and the Department of Defense. One of the first public announcements made by the Administrator after NASA was established was to the effect that NASA's procurement procedures would conform in every practicable way to the Armed Services Procurement Regulations. Thus industry was given the assurance at the very beginning of our operations that it would not have to become acquainted with an entirely new set of procurement regulations in order to do business with this new agency.

Several Executive Orders have also been issued which emphasize NASA's position as an agency important to the national security. An example is the Executive Order which gave NASA the so-called V-loan authority to guarantee loans to Government contractors. The Defense Production Act specifically names the military departments and the Department of Commerce as "guaranteeing agencies," but it also authorizes the President to designate additional agencies which are engaged in procurement for the national defense to exercise this authority when "deemed . . . necessary to expedite production and deliveries or services under Government contracts for the procurement of materials or the performance of services for the national defense.'

In addition, NASA possesses priorities and allocation authority under the Defense Production Act and the Executive Order and delegations which have been issued thereunder. Under this authority, Project Mercury, our manned satellite project, has been assigned the highest national priority, right along with the ballistic missile programs of the Department of Defense.

All of these legal actions, both by the President and the Congress, recognize the importance of NASA to the national security. In our view, the entire program of NASA is designed to enhance the security interests of the United States and, in the deepest sense, does facilitate and promote the national defense.

Now let's look at the other side of the coin. The Congress has declared "that it is the policy of the United States that activities in space should be devoted to peaceful purposes for the benefit of all mankind." In furtherance of this end, the Congress established a civilian agency, the National Aeronautics and Space Administration, to be given responsibility for all of the nation's space activities except, in the terms of the Act, "activities peculiar to or primarily associated with the development of weapons systems, military operations, or the defense of the United States," which remain the responsibility of the Department of Defense. The creation of a civilian agency for this purpose was a legislative act having the most profound implications; henceforth it was to be clearly understood that it was national policy to emphasize activities in space for civilian, rather than military, purposes.

What, then, is the mission of this new civilian agency? The National Aeronautics and Space Act of 1958 directs that NASA shall "plan, direct, and conduct aeronautical and space activities," and then it proceeds to give this term "aeronautical and space activities" a three-part definition. These are, first, "research into, and the solution of, problems of flight within and outside the earth's atmosphere;" second, "the development, construction, testing, and operation for research purposes of aeronautical and space vehicles;" and third, "such other activities as may be required for the exploration of space." The first two, research into problems of flight and the development and operation of aeronautical and space vehicles for research purposes, are certainly not new, nor are they unique to NASA. The National Advisory Committee for Aeronautics and the armed services were deeply engaged in such activities for years before NASA was established. But the third of these, "such other activities as may be required for the exploration of space," projects us into an entirely new dimension of human activity. The exploration of space! For the first time in the long history of legislation these words have been written into a law. It is safe to predict that the world -- at least the legal world I can assure you -- will never be quite the same again.

The exploration of space, then, is NASA's specific mission, and it is a mission for which it is solely and exclusively responsible under the law. This is a mission just as unique to NASA as the military defense of the nation is to the armed services. And, I repeat, it is a mission of vital importance to the security interests of the United States.

Here I think it is desirable to try to clear up some confusion that exists concerning what is frequently referred to as the nation's space program. This program should more properly be termed the nation's space exploration program. It is a program consisting, in the terms of the law governing NASA's operations, of all activities designed to further the exploration of space. It is a program for which NASA is directly responsible. And it is a program which does not embrace the military uses of space.

For the armed services, space is properly not a program at all, but just another place where military functions can be performed for the defense of the nation. The military utilization of space, and the research and development effort directed toward that end, are integral parts of the total defense program of the United States. Military space projects are undertaken only to meet military requirements, and they presumably must compete in the military budget with alternative, and more conventional, means of accomplishing the same military objectives. For the military, the test must be: is it, all things considered, the most prudent way to expend our resources to achieve the best defense of the nation? The military has no obligation whatsoever to perform any part of its mission in space merely because space is there, and man now, for the first time in history, finds it accessible.

NASA, on the other hand, has been directed by law to plan, direct, and conduct such activities as may be required for the exploration of space. The exploration of space has thus become a kind of end in itself -- an end to be pursued without having to justify it either in relation to the defense of the nation or the economic benefits which may possibly flow from it.

The exploration of space comprehends all the means of expanding man's knowledge of the space environment -- by the conduct of scientific experiments in space, and by observation through the use of unmanned vehicles equipped with the most ingenious instruments man can devise. But it doesn't stop there. Ultimately, and essentially, it means the sending of man himself into space. It is the prospect of man escaping from his earthly environment that makes the whole business the most exciting enterprise of our age. It is the thought of man being projected into a totally alien environment, whether in orbit about the earth, or standing for the first time on the moon, or preparing to land on another planet, that stimulates the human imagination as nothing else has stimulated it since the first days of powered flight a half century ago.

If we bear these considerations in mind, I think we can see more clearly where we are going in space and why we are going there. I think, too, that we will understand better the shape and content and dimensions of our space exploration program.

Our program ultimately is determined not by the desire of scientists to investigate the ionosphere, to study the great radiation belts, to observe cosmic radiation, or to make more precise measurements of the shape of the earth, praiseworthy as these scientific activities may be. Neither will it be determined by the prospects of economic benefits such as improved weather forecasting and world-wide communications through the use of satellite systems, although these, toc, are highly desirable goals. In the first case, a purely scientific space program would not merit more than a small fraction of our total space expenditures if it had to compete for funds with all other scientific programs which hold promise of importantly enlarging the borders of human knowledge.

In the second case, a program designed solely to achieve economic benefits ought to be coldly calculated in terms of the ratio of probable benefits to probable costs before expending large sums of money on it; and no one today has been bold enough, or wise enough, to make solid predictions that the cost of the nation's space exploration program will be returned to the taxpayers in the form of utilitarian applications. Just as the military space projects must face the competition of alternative means of accomplishing the military task, so should the development of a communication satellite system be justifiable in economic terms. We are, of course, making every effort to derive such benefits from our space program at minimum additional cost, but it seems abundantly clear that we would not be talking seriously about financing any of these things were it not for the overriding demands of space exploration itself. Space exploration is bound to have numerous and unpredictable payoffs, but these are not the reason for pursuing it with urgency.

Where are we going in space? Our scientists and engineers have told us that there are no insurmountable scientific or technical barriers to sending a man on a round trip to the moon in another ten years or so, or to sending a manned expedition to Mars during the 1970's. These are the presently foreseeable goals of space exploration in our time, and these goals will shape our entire program and determine its dimensions. They are, in fact, already determining it. Project Mercury, our manned-satellite project, has been assigned the highest national priority. Why? Certainly not because of the purely scientific data which will result from this project -- surely not because of the prospect of achieving economic benefits from it. No, it is simply and purely because of the role of man in that project, and the tremendous promise it holds of giving him his first firm grasp on the endless ladder of space. When the first man returns to earth after circumnavigating the globe three times in four-and-a-half hours -- between breakfast and lunch we might say -- a new epoch in the human adventure will have begun; and everyone, from the simplest to the most sophisticated, will know it.

Project Mercury is NASA's biggest single effort today. Of our total budget for this fiscal year, about

15 per cent is being expended on this project. The total costs of the project over a 4-year period are likely to run around $350,000,000.

As you know, Project Mercury relies on the Atlas ICBM booster to place a manned capsule, weighing about a ton, in orbit at an altitude slightly in excess of 100 miles above the earth. This is the limit of our capability for manned space flight using presently available boosters developed in the military missile programs. But the needs of space exploration obviously do not stop there. The next step now appears to be manned flights around the moon -- circumlunar flights, in the vernacular of the space experts -- and perhaps the establishment of permanent near-earth space stations suitable for human occupancy. Such ventures will require boosters having a thrust of about a million-and-a-half pounds -- or roughly four times the thrust of the Atlas.

Here is where Saturn comes in. As presently conceived, Saturn involves the clustering of eight IRBM engines, of the type used in the Thor and Jupiter missiles, to produce this amount of thrust in the take-off stage. Various combinations of second and third stages will probably be employed in the later development of the vehicle. The initial version should be able to put satellite payloads measuring many tons into orbit and to place thousands of pounds of payload in the vicinity of the moon. With the anticipated growth development of Saturn, it should ultimately be capable of sending several men on a trip around the moon, and even placing them in a satellite orbit about the moon, before returning them to earth.

Saturn, as you are well aware, has been under development by the Army Ballistic Missile Agency at Huntsville, Alabama. Less than two months ago the President announced his intention of transferring the Development Operations Division of that agency from the Army to NASA. This action will also involve transferring responsibility to NASA for continuing the development of Saturn. The President's announcement made it clear that this action was designed to consolidate in one agency, NASA, the development of all "super-booster" space vehicles, meaning all vehicles of very high thrust beyond those based on the current ballistic missiles and growth versions of those missiles. This is a most significant recognition of the importance of space exploration in the total national picture. The Saturn project simply could not have survived as part of a military program, since the necessary military justification was lacking. But as part of the nation's space exploration program, it will move ahead as a top priority project. During the next fiscal year, almost 20 per cent of NASA's total budget will be expended on pushing Saturn toward completion. Even so, this vehicle system will probably not be available for use until the 1964-65 period.

The next step is the development of a vehicle which will enable man to land on the moon and then return safely to earth. As the first step toward the achievement of such a vehicle, a contract was awarded to North American Aviation's Rocketdyne Division last January for the development of the F-1 engine. This is a single-chamber rocket engine designed to produce a million-and-a-half pounds of thrust -- the equivalent in one engine of the thrust created by the entire cluster of engines in Saturn. By clustering this monster of an engine, and adding upper stages, a vehicle can be produced which for the first time will make possible a manned lunar landing and return.

Both of the vehicles which I have described will have other uses, of course. Their tremendous weight-lifting capabilities will permit us to place large useful payloads far into space to land robot vehicles on the moon and nearby planets and to place scientifically effective payloads into orbits closely approaching the sun.

I think I should mention, in connection with these vehicles, that our Administrator, Dr. Glennan, recently announced that the group which has come to NASA from the Army at Huntsville will take over technical management responsibility for the F-1 engine development at Rocketdyne. Thus we shall have centered in one group the total job of managing the program for development of "super-boosters." At the Jet Propulsion Laboratory in Pasadena, which was transferred to NASA from the Army last December, we shall concentrate on lunar and interplanetary programs.

At Goddard Space Flight Center, now under construction at Greenbelt, Maryland, we will center the responsibility for earth satellite programs and for Project Mercury. And on Tuesday, Dr. Glennan announced the appointment of Major General Don Ostrander to head a new unit at NASA Headquarters charged with the management of our entire booster development program.

With the planned development of vehicles which will equip man to explore the moon, we come to the end of the next decade. During this period, if we vigorously pursue research on nuclear and electric rockets, we should move far down the road toward development of radically new methods of propulsion which will enable us to plan for manned expeditions to the nearer planets in the following years.

The goal of manned flight to the moon, and ultimately to the planets beyond, will affect the NASA program more than any other factor during the coming decade. The total NASA program is being supported by a budget for the current fiscal year of $501,000,000, of which almost 70 per cent is being expended through the contracting route. I wish I were at liberty to disclose the size of the budget which we will be presenting to Congress shortly for fiscal year 1961. Unfortunately, I am only able to say that it will be considerably greater than this year's budget. The costs of a vigorous program of space exploration will inevitably continue to grow.

Is there any real alternative? It hardly needs to be said that if we don't pursue with vigor the presently foreseeable goals of space exploration, there are others who will. Is it conceivable that the American people will be content to be onlookers ten and twenty years from now while another nation, eager to seize all the symbols of world leadership, triumphantly parades before the world the Magellans of space? We cannot forget that it was only three decades ago that a young American held the center of the whole world's stage by the first successful flight from New York to Paris; and in the following years, one intrepid American after another demonstrated in the most dramatic fashion the superior skill, technology, and, yes, the courage of this nation by pioneering flights all across the earth.

The spirit of the American people is still the same today, and they will expect and be satisfied with nothing less than leadership by the United States in this great new venture. If we keep our sights on the real goals of space exploration -- 5, 10, and 20 years ahead -- and pursue them with steadfastness, we can be confident of success.

NASA Release No. 59-272

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

NASA RELEASE NO. 59-271
Du. 2-7807

FOR RELEASE:
Friday P.M's
December 11, 1959

NASA CONTRACTS FOR OCTOBER

The National Aeronautics and Space Administration let the following new contracts during October, 1959:

Massachusetts Institute of Technology -- $70,000 -- Basic research to provide a better understanding of the noise phenomena associated with rocket launches and jet aircraft.

University of Cincinnati -- $180,000 -- Advanced mathematical research into celestial mechanics.

Ball Brothers Research Corp. -- $250,000 -- Instrumentation for a Delta-launched earth orbiting solar observatory. The $250,000 represents initial funding; total cost of the contract may reach $750,000.

Army Ordnance Missile Command -- $150,000 -- Initial funding for design, construction and integration of a Juno II-launched satellite to study the energy and source of gamma rays. Total contract cost may run $800,000.

AOMC -- $150,000 -- Initial funding for design, construction and integration of a Juno II-launched satellite to sample the ionosphere. Total contract cost may run $750,000.

National Bureau of Standards -- $130,000 -- Ground experiments with radio signals to determine properties of the ionosphere.

McGraw-Edison Co. -- $80,000 -- Repairs to rotor in unitary wind tunnel at Langley Research Center.

Navy Bureau of Ordnance -- $360,000 -- Purchase of 19 solid propellant rocket motors to be used as upper stages in a modified Scout (Project 609A) by the Air Force in high-altitude and re-entry tests.

Navy Weapons Plant -- $50,000 -- Shop work and materials going into mockups for Goddard Space Flight Center.

National Bureau of Standards -- $70,000 -- Use of computing system services.

Jones-Mahoney Corp. -- $110,000 -- Construction of buildings to house a new NASA minitrack station recently opened at Fort Myers, Fla. This station will do the work of two minitrack stations, one at Fort Stewart, Ga., and another at Havanna, Cuba, both of which NASA has closed.

Naval Research Laboratory (ONR) -- $70,000 -- Logistics and support in connection with work on the 100-foot sphere communications satellite to be launched by a Delta.

- END -

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

NASA RELEASE NO. 59-277
Du. 2-6325

December 15, 1959

MEMORANDUM TO EDITORS:

The attached document is designed as background information on U. S. and U. S. S. R. space sciences programs, including sounding rockets, probes and satellites. It was prepared by Dr. Homer E. Newell Jr., Assistant NASA Director for Space Sciences.

U. S. AND U.S.S.R. SPACE SCIENCE RESULTS

Homer E. Newell, Jr.
Assistant Director, Space Sciences
National Aeronautics and Space Administration

Results Obtained.

The United States has been using sounding rockets for upper air research and rocket astronomy since the close of World War II. WAC Corporal, V-2, Viking, Aerobee, Aerobee-Hi, Nike-Deacon, Nike-Cajun, Nike-ASP, and Rockoons used. Altitudes attained were below 200 miles for the most part. Many hundreds of rockets were fired prior to the start of the International Geophysical Year; an additional 200 were fired as part of the International Geophysical Year program. Current rate of rocket soundings is somewhat below 100 per year. Higher altitude rockets are being introduced into the work to extend the atmopsheric observations to one to several thousands of miles altitude. Launchings have been carried out at White Sands, New Mexico; Wallops Island, Virginia; San Nicolas Island, California; Cape Canaveral, Florida; Fort Churchill, Canada; Guam; and from shipboard in the North Atlantic, the Mid- and South Pacific, and the vicinity of Antarctica.

The United States program has produced hundreds of research papers and reports giving results on the pressure, temperature, density, winds, and composition of the upper atmosphere, the ionosphere; the earth's magnetic field; the aurora and airglow; cosmic rays; micrometeors; solar radiations; and ultraviolet astronomy. Some experiments have been carried out on modifying the upper atmosphere by the release of special chemicals, and on modifying the radiation belt by nuclear explosions. Some bio-sciences experiments have been performed.

The USSR has also been carrying out a rocket sounding program since the last war. Although the precise number of rocket soundings to date is not known, they number in the hundreds. Firings have been made from Franz Josef Land and from Mirny in Antarctica, as well as from European USSR. The Soviets have perfected a meteorological sounding rocket that is used for more or less routine soundings of the atmopshere to measure air pressures, densities, and temperatures up to 35 miles altitude. In addition, their "geophysical rocket" is capable of carrying ton and a half payloads up to 300 mile altitudes.

From their sounding rocket program the USSR has obtained a broad collection of results. The meteorological soundings have produced detailed data on the structure of the upper atmosphere just above the troposphere, showing its temporal and seasonal variations. The geophysical rocket program has provided considerable information on the very high atmosphere, including the inosphere. The description of one of the geophysical rocket payloads is so similar to the description of Sputnik III and its instrumentation as to lead one to conjecture that the payload may have been essentially the Sputnik III payload. Whether or not this is the case, the instrumentation provided for a broad range of measurements on the ionosphere, atmopsheric structure, energetic particles, and the earth's magentic field. The USSR rocket program has also included considerable work on biological researches. There have been some 20 tests in which dogs, and/or rabbits were sent aloft and recovered for study. During the flight the behavior of the animals was telemetered to ground.

The USSR launched the first successful artificial earth satellite. To date the USSR has successfully launched 3 earth satellites, and 3 space probes. Two of the space probes achieved earth escape velocity; the first passed within 2 or 3 moon diameters of the moon. The second Soviet space probe actually hit the moon. The third space probe was launched so as to pass close enough to the moon to take pictures of the unseen side of the moon's surface, then to loop around the moon returning to the earth. The lunar pictures were successfully obtained.

The United States has to date successfully launched 15 earth satellites; namely 5 Explorers, 3 Vanguard, Project Score, and 6 Discoverers; and 3 space probes, all called Pioneers. Only one of the space probes achieved earth escape velocity, passing by the moon at some 37,000 miles distance.

Both the United States and Soviet satellites and space probes have produced valuable scientific results. Included are some spectacular discoveries and achievements, some of which are given in the accompanying Table No. 1. In addition to the more spectacular output, these satellite and space probe flights are turning out a steady flow of information and results that build up gradually to an impressive advancement of mankind's knowledge of the earth and outer space. Some of these are listed in Table 2.

Problems Being Attacked.

In attempting to compare the relative stages of advancement of the US and USSR in space research, one might proceed by trying to list item by item the individual results from the two programs and to relate these results item to item. This would turn out to be difficult even if one were sure that all the results obtained by the Soviets were actually at hand, for there would be many observations obtained by the Russians that had not yet been obtained by the United States, and conversely, many obtained by the US that had not yet been obtained by the Russians. A more effective, and perhaps more significant way of comparing the relative stages of advancement, would be to isolate the general areas of investigation and the general problems being attacked by the two countries.

Taking this approach one can say that the US and the USSR appear to be at about the same stage of advancement in upper air research. The US results on the atmopshere below 200 miles appear to be more detailed and complete, but the Soviets have made higher altitude measurements by means of their geophysical rocket. The Soviets appear to have done far less than the US on solar radiations, but the USSR has done much more than the US on bio-sciences experiments, having conducted numerous flight tests in which dogs were carried aloft in rockets and safely recovered.

The USSR has carried the technique of ejecting instrumented packages from the rocket carrier farther than has the US, which has carried the technique of telemetering to a high degree of refinement.

Likewise, the US and the USSR seem to be at about the same stage of advancement in studies of the earth's environs where satellite techniques are adequate for making the necessary observations. In fact it may be that in this regard the United States has the slight edge. The big advantage the Soviets have in attacking these problems lies in their greater payload capacity. On the other hand, the United States has launched many more satellites than the Soviet Union.

In deep space probe work the USSR has definitely taken the lead. This is directly attributable to their clear lead in vehicle technology.

Table 3 provides a comparison of the states of advancement of the United States and the USSR.

A review of Table 3 shows fairly clearly that the United States and the USSR scientists are at about equal stages of advancement in the problems they are attacking or are about to attack in space research. As groups they undoubtedly have comparable competencies and understandings of the significant problems that ought to be tackled. Their instrumentations are roughly equivalent, although the United States may have a slight edge here, as indicated by the fact that the USSR quite often simply copies United States equipment for its own instrumentation. The conclusion follows then that the side that has the more advanced technology in the way of payload capabilities, guidance, etc., will have the distinct edge and by virtue of the increased flexibility and capabilities provided by the more advanced technology will force steadily ahead. Thus, one may predict a time lead in vehicle technology will be transformed into a corresponding time lead in the exploration and investigation of outer space.

Table 1

SIGNIFICANT FIRSTS IN SOUNDING ROCKET, SATELLITE, AND SPACE PROBE RESEARCH

United States

1. A number of firsts in high altitude rocket research, including among others:

    First detailed photo of solar ultraviolet spectrum.

    First photo of complete tropical storm.

    First penetration of equatorial ionospheric current sheets.

    First detection of X-rays in high atmosphere.

    First detection of auroral particles in high atmosphere.

2. Discovery of the Van Allen Radiation Belt.

3. Discovery that the Van Allen Radiation Belt consists of at least two zones.

4. Performance of the Argus experiments.

5. The first precise geodetic use of artificial earth satellites (Vanguard I) to obtain refined information on the size and shape of the earth, providing an improved value for the flattening and showing that the earth is actually slightly pear shaped.

6. First achievement of an elementary communication satellite, in Score.

U. S. S. R.

1. First artificial earth satellite.

2. First lunar near miss.

3. First lunar impact.

4. First pictures of the hitherto unseen side of the moon.

5. First detection of what may be a current ring about the earth (the Chapman-Størmer ring).

U. S. S. R. (Continued)

6. First routine recovery of large animals (dogs, and rabbits) from high altitude rocket flights.

7. Development and routine use of a meteorological sounding rocket, recoverable and reflyable.

8. First launching of a large animal (Laika) in a satellite of the earth.

9. First high capacity, maneuverable, heavily instrumented, spacecraft with fully successful long range communications (Lunik III).

Table 2

SOUNDING ROCKET, SATELLITE, AND SPACE PROBE RESULTS

FIELD	UNITED STATES	U.S.S.R.
Upper Atmosphere	1. Rocket observations have been made of pressure, temperature, density, composition, and winds of the high atmosphere at a wide variety of locations, both day and night, and in the various seasons.	1. Rocket observations have been made of pressure, temperature, density, composition and winds of the high atmosphere at a wide variety of locations, both day and night, and in the various seasons.
	2. Upper air densities have been obtained from the tracking of both US and USSR satellites.	2. Upper air densities in the higher latitude regions obtained from drags on Sputniks I and III.
	3. It has been shown that the radiation belt may account for much higher atmospheric temperatures observed in the auroral zone atmosphere than in the high atmosphere above the middle and equatorial regions.	3. High enough flux of low energy electrons measured with Sputnik III instruments in the northern regions to account for the higher atmospheric temperatures there.
	4.	4. Direct measurement of upper air densities made with gages in Sputnik III, for heights up to 355 km.
	5. Fluctuation in satellite drag, hence presumably upper air densities, have been shown, from observations on Vanguard I and Sputnik II, to be directly correlated with fluctuations in the 10 cm radiation from the sun, and hence solar activity.	5.

Table 2 (Page 2)

FIELD	UNITED STATES	U.S.S.R.
Upper Atmosphere (Contd)	6.	6. The routine meteorological sounding rocket has been used to give atmospheric structure data at middle-European, Arctic, and Antarctic locations showing seasonal variations as well as geographic. It turns out that the seasonal variations are different for the different altitude ranges.
	7. From both satellite and rocket observations high altitude air densities have been shown to vary widely with time of day, season, and geographic position.	7.
	8. The amounts of diffusive separation both below and above the E region of the ionosphere have been measured in sounding rocket experiments, and shown to be very slight below the E region and quite pronounced above altitudes of 110 to 120 km.	8. Diffusive separation in the upper atmosphere below the E region has been measured with results that agree in general with the US observations.
Ionosphere	1. Extensive electron density data have been obtained for a number of locations from rocket soundings.	1. From rocket soundings electron densities have been obtained up to and above the F region maximum.
	2. From radio signals of both US and USSR satellites, propagation characteristics of the ionosphere and electron density distributions have been obtained.	2. Electron densities above 300 km were obtained by observation of the radio signals of Sputniks I and III.
	3.	3. Observations on Sputnik I showed 3.5 times as many electrons above the F region maximum as below.

Table 2 (Page 3)

FIELD	UNITED STATES	U.S.S.R.
Ionosphere (Contd)	4. The heavy ions in the ionosphere above White Sands and Fort Churchill have been identified up to the F region in rocket sounding experiments.	4. The ionic composition of the ionosphere has been measured in sounding rockets to above the F region maximum.
	5.	5. Sputnik III observations showed that the predominant ion from 250 to 950 km is positive atomic oxygen, O^+.
	6.	6. In Sputnik III the satellite potential in the daytime ionosphere was observed to be as much as -7 volts.
	7. Very low frequency propagation data were obtained from Explorer VI.	7.
	8.	8. In the second Lunik, evidence of a lunar ionosphere was obtained.
Magnetic Field	1. Data on earth's magnetic field were obtained from Pioneer I and Explorer VI, and a great deal of additional high quality data are being obtained from Vanguard III.	1. Data on earth's magnetic field obtained from Sputnik III.
	2. By their magnetic effect, electric current flows were plotted in the E and lower F regions, in rocket sounding experiments in the equatorial regions.	2.

Table 2 (Page 4)

FIELD	UNITED STATES	U.S.S.R.
Magnetic Field (Contd)	3.	3. On Mechta measurements were made of the earth's magnetic field and its extension into space. A marked dip in the field was discovered in the region of the radiation belt, indicating perhaps the existence of a current ring such as postulated by Chapman.
	4. Rocket measurements of the earth's magnetic field have been made in the auroral regions	4.
	5.	5. Lunik II, on its plunge to the surface of moon, showed that the lunar magnetic field is not greater than 50 gamma.
Cosmic Rays	1. Extensive data on cosmic ray intensities, composition, and interactions with matter were obtained from sounding rockets in various locations and throughout all the seasons.	1. Cosmic radiation measurements have been made in USSR sounding rockets.
	2. The cosmic ray count was obtained above the atmosphere with counters in Explorer satellites and Pioneer probe.	2. The cosmic radiation was measured in Soviet satellites and space probes.
	3. Cosmic ray counts in the first Explorers gave discovery of the Radiation Belt.	3. Sputnik II observations showed an increase in counting rate with height (this being at the time, an unrecognized hint of the presence of the radiation belt).

FIELD	UNITED STATES	U.S.S.R.
Cosmic Rays (Contd)	4. Details on the cosmic radiation as a function of time and position in space have been obtained from Explorer VI, and are being obtained from Explorer VII.	4. Sputnik III and cosmic rockets provided measurements on the heavy nuclei in the cosmic radiation.
Radiation Belt	1. Radiation belt discovered with instruments in Explorer I.	1. Abnormally high cosmic ray counts were observed in Sputnik II, particularly at the high latitudes. Sputnik III showed a very high electron flux in the northern latitudes.
	2. A great amount of additional detail obtained on belt in Explorers III and IV, and the Pioneer probes. Extent of radiation belt shown by Pioneer I. Pioneer III showed belt to consist of at least two zones.	2. Sputnik III, Mechta, and other Soviet satellite and space probe observations confirm the US findings.
	3. Pioneer IV showed the extent of the outer radiation belt to have increased greatly following a five-day period of high solar activity, thus proving that the outer belt is of solar origin.	3.
	4. Argus experiments showed individual inner zones of the radiation belt to be very stable.	4.
	5. Argus observations lends support to conclusion that inner radiation belt produced by cosmic rays. See No. 7 below.	5.

FIELD	UNITED STATES	U. S. S. R.
Radiation Belt (Contd)	6. Detailed energy spectrum of radiation in radiation belt was obtained by Explorer VI.	6.
	7. Sounding rocket observations showed that the energetic particles of the inner radiation belt are protons of energy spectrum expected from β decay of neutrons, hence supports cosmic ray origin for hard components of inner belt.	7.
	8. Extensive additional information on the radiation belt was obtained from Explorer VI and is being obtained from Explorer VII and Vanguard III. Hugh variations of many orders of magnitude in counting rates were observed in outer zone.	8.
	9. Radiological hazard of radiation belt estimated to be not serious for a direct traverse of the belt; but quite serious for a space station that spends a lot of time in the belt.	9. Radiological hazard of radiation belt estimated to be not serious for a direct traverse of the belt; but quite serious for a space station that spends a lot of time in the belt.
	10.	10. The moon was shown not to have a radiation belt detectable within the sensitivity of Lunik instruments.
Aurora	1. Rocket soundings have been used to study the electromagnetic and particle radiations in the aurora. It was found that soft radiation flux above 40 km was many times the primary cosmic ray count.	1.

Table 2 (Page 7)

FIELD		UNITED STATES		U.S.S.R.
Aurora (Contd)	2.		2.	A very high flux of low energy electrons was observed in Sputniks II and III. This flux was taken to be the cause of the very high atmospheric temperature in these regions.
	3.	The particles in the outer radiation belt have been shown to be the likely immediate cause of the aurora.	3.	The particles in the outer radiation belt have been shown to be the likely immediate cause of the aurora.
Geodesy	1.	Vanguard I observations give an oblateness of the earth of 1/298.3.	1.	
	2.	Vanguard I observations show the earth to be pear shaped with a 50 foot peak at the north pole, and a 50 foot flattening at the south pole; this appears to imply an internal strength to the earth, rather than a free flowing plasticity.	2.	
Meteors	1.	A fairly low count of micrometeors corresponding to a total influx of 1,000 to 10,000 tons of material per day, from Explorer and Pioneer observations.	1.	Influx of material per day indicated by Sputnik III observations in general agreement with the US results.
	2.	A very large amount of additional data are being obtained from the Vanguard III instrumentation.	2.	Additional measurements made in Soviet cosmic rocket flights.

Table 2 (Page 8)

FIELD	UNITED STATES	U.S.S.R.
Astronomy	1. In sounding rocket experiments ultra-violet sources in the sky have been detected and plotted.	1.
	2. The solar spectrum has been observed and photographed down to 303 Angstroms.	2.
	3. Solar radiations have been observed and measured in the X-ray regions.	3.
Lunar Explorations	1.	1. First photos taken of the hitherto unseen side of the moon.
	2.	2. The lunar magnetic field shown to be no greater than 50 gamma.
	3.	3. Lunar ionosphere detected.
Miscellaneous Experiments	1. The Argus experiments were carried out.	1.
	2. Sodium vapor was released in the high atmosphere and observed to measure its radiations, atmospheric winds, and diffusion.	2. Sodium clouds were released from Luniks II and III and observed from the ground.
	3. Various chemical contaminants were released in the high atmosphere to study the photochemical reactions that resulted.	3.

Table 2 (Page 9)

FIELD	UNITED STATES	U.S.S.R.
Biosciences	1. On numerous sounding rocket flights biological specimens of seeds, fruit flies, etc. have been flown and recovered for study. Larger animals, such as rats and monkeys, have been flown for study of their behavior and the effects of the flight environment on them. Recovery of such animals has been effected on numerous occasions.	1. Large numbers of sounding rocket experiments have been carried out with dogs and rabbits, in which the animals were both studied during flight and recovered after flight for further study.
	2.	2. Observations were made on the behavior of Laika, particularly heartbeat and respiration, in Sputnik II.
Engineering Data	1. US satellites show that moderate temperatures can be achieved in orbiting vehicle.	1. USSR satellites and space probes show that moderate temperatures can be achieved by appropriate engineering.
	2. Elementary communications link checked out in Project Score.	2.
	3. Based on radiation belt data, it is deduced that satellites may charge to a potential of some hundreds of volts in the radiation belt.	3. Sputnik III measurements show that in the daytime ionosphere the satellite acquired an appreciable negative charge corresponding to a negative potential of several volts.
	4. The meteor erosion and puncture problems have been shown in general to be not particularly serious.	4. The meteor erosion problem appears to be not particularly serious.
	5. An elementary TV scanner was checked out in Explorer VI, while some of the basic elements of a meteorological satellite were checked out in Vanguard II.	5.

Table 2 (Page 10)

FIELD		UNITED STATES		U.S.S.R.
Engineering Data (Contd)	6.		6.	Automatic photography of the moon and the televising of the photographs obtained back to earth has been achieved.
	7.	Solar cells have been shown to be a practical, reliable source of power.	7.	Solar cells have been shown to be a practical, reliable source of power.
	8.		8.	A complete spacecraft, maneuverable, with temperature control, power supply, long range communications link, complicated instrumentation, etc. has been engineered and flown successfully, -- namely, Lunik III.
	9.	It appears that the radiological hazard to space vehicle crews traversing the radiation belt directly may be relatively low, while the hazard to those in a satellite orbiting through the radiation belt would be quite serious. In addition, marked increases in proton intensities of the cosmic radiation found at the time of solar activity may be a very serious radiological hazard: dose rates of 1000 r/hr.	9.	It appears that the radiological hazard to space vehicle crews traversing the radiation belt directly may be relatively low, while the hazard to those in a satellite orbiting through the radiation belt would be quite serious.
Meteorology	1.	Numerous sounding rocket photos of cloud formations and significant weather areas have been taken. In particular a composite photo from one sounding rocket showed a completely developed tropical storm approaching hurricane proportions.	1.	

Table 2 (Page 11)

FIELD	UNITED STATES	U.S.S.R.
Meteorology (Contd)	2.	2. A meteorological sounding rocket was developed and has been used on a routine basis for meteorological studies.
	3.	3. Detailed measures of pressures and temperatures have been obtained with the meteorological rocket for Antarctic, Arctic, and Middle European locations.
	4. Cloud picture data were obtained in Vanguard I, but motions of the satellite have so far prevented reducing the data to useful pictures. Also, very low resolution, elementary television pictures have been taken of cloud formations as seen from Explorer VI. One of these pictures was assembled and released.	4.

Table 2 (Page 11)

FIELD	UNITED STATES	U.S.S.R.
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Table 3

PROBLEMS CURRENTLY UNDER ATTACK

FIELD	UNITED STATES	U.S.S.R.
Upper Atmosphere	1. A detailed study of the structure, winds, and composition of the ionospheric regions and beyond in the earth's atmosphere is underway by means of sounding rockets and earth's satellites.	1. A detailed study of the structure, winds, and composition of the ionospheric regions and beyond in the earth's atmosphere is underway by means of sounding rockets and earth's satellites.
	2. Work is underway to develop a routine rocket sonde for synoptic studies of the lower portion of the upper atmosphere in association with meteorological soundings.	2. The USSR has already achieved the development of a routine rocket sonde for meteorological type soundings into the lower portion of the upper atmosphere.
Ionosphere	Intensive rocket and satellite studies of the ionosphere in the F region and beyond are underway.	Intensive rocket and satellite studies of the ionosphere in the F region and beyond are underway.
Magnetic Field	The US has used search coils saturable core magnetometers and proton precession magnetometers in its measurements of the earth's magnetic field. The US is preparing to use a much more sensitive instrument, the alkali vapor resonance magnetometer, for further studies of magnetic fields in space and to measure the magnetic field of the moon.	The USSR has also used standard type magnetometers and proton precession magnetometers for observations of the earth's magnetic field. The USSR has made a measurement to detect the lunar magnetic field, finding none to within the sensitivity of their instrument. It is not known whether the USSR is preparing to use the alkali vapor magnetometer in the near future.

Table 3 (page 2)

FIELD	UNITED STATES	U.S.S.R.
Cosmic Rays	Balloon, sounding rocket, and satellite observations of the intensity, nature, and effect of cosmic rays are underway.	Balloon, sounding rocket, and satellite observations of the intensity, nature, and effect of cosmic rays are underway.
Radiation Belt	Detailed study of the Radiation Belt by means of sounding rockets, satellites, and space probes, with occasional use of controlled experiments is underway.	The USSR made intensive studies of the Radiation Belt in Sputnik III, but at the present time appears to be investigating the belt incidentally as part of their concentration on deeper space missions, namely on their Lunik flights.
Aurora	US scientists are tackling the problem of both visible and ultraviolet auroral radiations, the particles connected with the aurora, and the ultimate origin of the aurora.	The USSR scientists are tackling the same problems.
Geodesy and Celestial Mechanics	US is continuing use of satellites for geodetic studies.	The USSR shows skill in applications of celestial mechanics, as witnessed by their ability to launch Lunik III with the accuracy achieved, and to predict the motions of the Lunik III spacecraft.
Meteors	The US continues to collect data on meteors in space, using a wide variety of experimental equipments.	The USSR has made an intensive study of micrometeors in their satellites and space probes, appearing to attack the general problem very much along the lines followed by the US.

Table 3 (page 3)

FIELD	UNITED STATES	U.S.S.R.
Astronomy	Active rocket astronomy in being. Orbiting telescopes, solar, and astrophysical observatories being worked on.	Unknown.
Lunar Exploration	The US is preparing to conduct intensive investigations of the moon, but the actual observation of the moon from space vehicles is yet to begin.	The USSR has already achieved significant steps in its investigation and study of the moon. It may be presumed that the Soviets will continue their vigorous efforts in this area.
Planetary Investigations	US has minimal capability in this area at present, and on the present schedule planetary work is proceeding at a very low pace.	The USSR has an advanced capability in this area, and has declared its definite interest in planetary research.
Miscellaneous Experiments	US using upper atmosphere regions for controlled chemical and Argus type experiments. Also planning relativity and gravity experiments.	Unknown.
Biosciences and Man-In-Space	The US has a first stage man-in-space program in project Mercury. Support work of a research type is being carried out in the Discoverer program. Some experimental work is being carried out in sounding rocket flights. A well rounded, fully developed program of research in both bio-technology and biosciences is yet to be worked out.	The USSR has a highly active program of research on animals under rocket flight and satellite conditions. It is not known how fully developed their bio-technical and fundamental biosciences programs are. It is expected, particularly from recent news releases, that the USSR does have a man-in-space program.

Table 3 (page 4)

FIELD	UNITED STATES	U.S.S.R.
Meteorology	The US is developing rocket photographic techniques for meteorological purposes. The US is developing a meteorological sonde for synoptic soundings. The US is conducting fundamental satellite experiments associated with meteorology, and is taking the initial steps in the development of a meteorological satellite system.	The USSR has already developed a working meteorological rocket sonde, which they have already put to extensive use. It is not known what the USSR is doing in the matter of developing a satellite meteorological system.
Communications	In its rocket, satellite, and space probe telemetry the US has shown good capability. Long range communication systems are being worked on for deep space probes. Communication satellite systems are being worked on.	The USSR rocket, satellite, and space probe telemetry has been successful. In particular the communications and telemetry problems of Lunik III appear to have been worked out with a high degree of competence. It is not known whether they are developing a communication satellite system, but it may be presumed that they are.
Navigation	The US is working on a navigation satellite of high degree of refinement.	It is not known whether the USSR is devoting effort to a navigation satellite.

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

NASA Release No. 59-279

ASTRONAUTS PRESS CONFERENCE
December 15, 1959

4:30 p.m.

NASA Flight Research Center
Edwards, Calif.

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PRESENT: PAUL F. BIKLE, Director of NASA Flight Research Center

MAJ. WILLARD R. HAWKINS, USAF School of Aviation Medecine

JOHN POWERS, Public Affairs Officer, Space Task Group, NASA

MATTHEW H. PORTZ, Western Information Officer, NASA Western Operations Office

ASTRONAUTS: M. SCOTT CARPENTER

ALAN B. SHEPARD

MR. PORTZ: Ladies and gentlemen, I think everyone is here, so I think we might as well start in. I am Matt Portz, the public affairs officer for NASA, and I believe most of you know Shorty Powers, public affairs officer for Project Mercury. This is Shorty's show and Project Mercury's show, so he will take over from here.

POWERS: The name tags are out in front of this distinguished group of gentlemen. I don't think Paul Bikle really needs any introduction to the people here in the local area; he is the director of what used to be the high speed flight station and now the flight research center for NASA. He has been generous and kind enough to let us occupy a whole bunch of his office space and telephones and lots of other things around here for the last three weeks. What we would like to do, we have already been out to the airplane today, this afternoon, to let you get some pictures if you were interested in doing so. As we proceed through this program, I will be asking Dr. Willard Hawkins, who is from the School of Aviation Medicine, to give you a run-down on the kind of zero G flying that the Astronauts have been doing while they are here. The pilots of the aircraft are here, if you would like to talk to them. Captain Richardson is the officer who owns and operates that trailer you saw out in the back yard, so that if any of you have any questions or anything that you would like to talk about concerning the trailer, you may do so, and I have two sparkling -- actually, not so sparkling, not at the moment, because I think they are worn out from being in the pressure suits -- Mercury Astronauts. The other five have already been here and they have been in this training program now for three weeks, and we are rounding it up or ending it up this week. Without a lot of other words, I would like to introduce Mr. Paul Bikle.

MR. BIKLE: I have a rather easy task this evening. I would like to welcome the members of the press to the NASA flight research center. I suspect we will be seeing more of each other as the X-15 research program develops. Although we are hosts to this meeting and the NASA Mercury Astronauts training program is an NASA program, I think we ought to point out that this zero G phase of it is conducted by the Air Force School of Aviation Medicine and with considerable help from the Air Force Flight Test Center people across the base here. I would like to urge all of you to call on either or both Matt Portz and myself in the future if there is anything we can do to help in giving you any information that you may need on the country's aeronautic and space program. You, Shorty, do you want to say something else, you want to talk, or just to go ahead with this?

POWERS: Yes, I would like to bring you up to date, if I may, on some of the things that have been going on in our training program. We were here on the 18th of September and had a conference, as I think most of you know, out at the Ballistic Missile Division. First, as far as the academic training is concerned, I think that it could safely be said that we have proceeded from basics on into more advanced subject matter. For example, in the case of basic astronautics, we have progressed from theories of propulsion and guidance and what have you to a study of the actual propulsion and guidance systems that will be used in the birds that are going to boost the Mercury capsule in its flight. As far as the travel program is concerned, very quickly, we have visited the McDonnell Aircraft Corporation, the prime contractor for the Mercury capsule. We have been to Huntsville, Alabama, where I think most people know the Redstone missile is being manufactured, or at least managed from there; and each of the Astronauts spent a week at the Air Force Wright Air Development Center at Dayton where they had an opportunity to get a basic introduction to full pressure suits. That included work in pressure chambers and heat chamber and centrifuge work. Some of the Astronauts at that time got an opportunity to do their first zero G flying in this program in the back of an Air Force C-131 type aircraft. That zero G was for very short periods of time, 12 to 14 seconds, and was more in the form of an introduction to zero G or weightlessness than anything else.

Each Astronaut has spent a week at the Naval Medical Research Institute in Bethesda, Maryland. During that period the Astronauts participated in familiarization with an atmosphere containing high carbon dioxide. In addition each Astronaut spent a period of hours in a heat chamber identified by the Navy as a human calorimeter. This session might well be likened to the air-conditioning engineer who studies the room or the building or the facility he is going to air-condition, and it was a study of the thing that needed to be air-conditioned, namely, astronaut types, to find out how hot they got and how they felt when they did get hot. The Astronauts paid a two-day visit to the Air Force Missile Test Center at Cape Canaveral, Florida. I think that this trip early in the program might best be described more as a tourist kind of a function. It was an effort to introduce them to Cape Canaveral to see the launch facilities and meet the people who would be involved in managing the launch operations at Cape Canaveral.

In September we had a three week trip out here to the west coast and included two days here at Edwards, with familiarization with the X-15 program. We got a chance to meet Scott Crossfield, Joe Young -- Joe Walker, rather, Bob White, Neil Armstrong, and all of the people here at the NASA Flight Research Center who are working on the X-15 program. I think probably one of the most interesting parts of that particular trip was about a half day long shirtsleeve closed door session between, at that time, only six Mercury Astronauts and the X-15 pilots, at which time they compared notes and experiences; for example, their experiences on the centrifuge at Johnsville, Pennsylvania; and I think they all came away with perhaps a little more knowledge about the total program.

We spent two days on that same trip with the Air Force Ballistic Missile Division in Los Angeles being briefed by General Ritland and his people on the Atlas program. We spent one day at the Rocketdyne Division of North American Aviation Company out at Canoga Park. We got detailed information on the propulsion systems that are being used in both the Atlas and Redstone. We got a chance to get out to the Santa Susana

Mountains and see some test firings of rocket engines. Then in an effort to give the Astronauts more detailed information about the Atlas, we spent five days at Convair's manufacturing facility at San Diego. During that week they met with all of the management people and the engineers and the people that are actually soldering the wires and turning the nuts and bolts in the Atlas missile, and I am sure they came away with a considerably broadened knowledge of how the missile is put together.

Each of the Astronauts has now flown -- and that's in quotes -- approximately ten hours in the human centrifuge at the Navy's Aviation Medical Acceleration Laboratory at Johnsville, Pennsylvania. During the centrifuge training we were able to reproduce at least as far as G's or acceleration, positive acceleration, was concerned, the same profile that they will experience in their Atlas-boosted flights, both going out and coming back. These particular rides were not concerned with temperatures or pressures or any other factors other than G or acceleration. Each of the Astronauts has now visited the B. F. Goodrich Company facility at Akron, Ohio, and we have tentatively accepted delivery on seven prototype Project Mercury pressure suits. I would like to point out at this time that the pressure suit you saw out here that Scott Carpenter was in is not a Mercury pressure suit; it is a Navy Mark 4 suit that we are using during this training because it is not possible to plug in all of the ventilation and oxygen breathing and what have you the Mercury suit requires in this particular aircraft. The suit itself, the Mercury suit, is being developed by a team of people from the NASA space task group back at Langley, the B. F. Goodrich Company, and the Navy Aircrew Equipment Laboratory in Philadelphia, Pennsylvania. Each of the Astronauts has now been fitted with a custom-made prototype. We are proceeding with the work that is required to both train the astronauts and conduct continuing development work to produce a usable suit.

As soon as they got their suits they went directly to McDonnell Aircraft Corporation, where we molded new couches for each of the Astronauts. We had been using a couch that was molded while the Astronauts were essentially in a flying suit as Alan Shepard is now, and when we accepted delivery on the pressure suits there was a requirement to go back and remold a new couch to fit the man in the pressure suit. After the St. Louis visit each of the Astronauts went out to the Aircrew Equipment Laboratory at Philadelphia, the Navy installation, where they went through additional familiarization with the pressure suit. They worked on a mock-up of the Mercury instrument panel, performing pre-selected physical tasks for the dual purpose of making sure that they had a suit that gave them mobility and familiarizing the Astronaut himself with the kind of energies and forces he was going to be required to expend to perform his normal pilot functions in the Mercury capsule. This was done under one G, that is, normal G conditions. It was conducted under pressurized circumstances with the suit pressurized.

In addition, each Astronaut spent several hours in a rather unusual combination pressure and heat chamber at the ECL facility in Philadelphia. In this chamber it is possible to simulate a reentry into the earth's atmosphere beginning at an altitude of 60,000 feet down to the earth's surface, and creating realistically the conditions of both pressure and temperature on the time scale involved in the reentry process. This now leads us to the current phase of training here at Edwards Air Force Base with the School of Aviation Medicine at zero G, and I would like to ask Doctor Hawkins if you would explain to these people how we have been running the schedule, how many flights we've had, and what you have been finding out.

DR. HAWKINS: Very good, Colonel Powers. Well, I might preface my statements first, if I may, by pointing out that zero gravity is a condition which is certainly difficult to come to grips with. It is a very unique situation, one which man has certainly never before experienced except for very short, very brief portions of a second or possibly a second or two, as if diving off of a diving board or going over a roller coaster. It is impossible to simulate this condition on the surface of the earth in the laboratory, and so the operation becomes a very costly one. The time duration that we are able to achieve, although we feel like that this is a tremendous accomplishment when you speak of a minute or fifty seconds of weightlessness, this doesn't really seem like a long period of time. However, it is a considerable period of time when you realize that man has never before experienced this. I feel like, therefore, that the duration of which we are able to achieve in using the high performance aircraft like you saw out here, the F-100's, that this does give us a chance to evaluate the man's physiological responses during this weightless condition as well as his psychomotor performance, his thinking, and give us an insight into what the problems are going to be in a more prolonged period.

We are very happy, the School of Aviation Medicine, to have out here as our research team which consists of about six officers and an equal number of airmen who support our research studies. We are happy to have this opportunity really to participate and help out in the Mercury training phase -- zero gravity training phase. This is a big operation, really, and one point that I would like to get across and point out, which I think is really the most interesting aspect of all of it, is that here we have three large organizations in our country who are working together in a combined effort to accomplish a specific mission, and this I am referring to as the NASA group, which is one Federal agency; we have then the Air Force wherein we have two major commands participating here, and this is the ARDC command with primarily the Flight Test Center here giving the support that they are giving to this program; and Air Training Command, of which the School of Aviation Medicine is a part of.

I feel like that the time out here for the two weeks that we have been here now, almost three weeks, has borne out that a unified and combined effort definitely can be achieved. I feel that the program is going very well, myself. I am extremely happy with it. I feel that we have not only been happy to provide the Astronauts with some additional training, but that we have also had the opportunity here of collecting some very valuable and important physiological data that heretofore we have never been able to achieve. In fact, there is really -- this is really a first time, a first event wherein we have been able to collect this much physiological data from in flight, and this, I think, in itself is certainly a monumental achievement that has resulted from over a year's work out here between the School of Aviation Medicine and from the Flight Test Center here at Edwards.

Now, then, what we have attempted to do with the Mercury Astronauts is this. We are trying to give each one of them four flights. A flight is of about 50 minutes or 60 minutes' duration, almost an hour, from the time that they take off until they land. During that time we are able to fly about three or possibly four parabolic flight maneuvers, which is the maneuver that we have to fly in order to achieve the weightless condition. By flying these calculated maneuvers, then we are able to produce a weightless condition for about 50 to 60 seconds. Now, then, as I said before, the Mercury Astronauts got four flights, and they roughly get about three parabolas per flight, so all in all, to finish up the program out here, we will have flown about 36 flights.

Now, on the first flight we have set up the program so that we have a psychomotor work unit in the aircraft which the Astronaut is required to work throughout the duration of the weightless period. This gives us a measure, and we are recording his responses on board during the flight maneuver, this gives us a measure of his time, speed, and his accuracy in being able to reach for objects or reaching for a specific point while weightless. We want to know, is there a deviation from normal? Does he tend to overshoot, undershoot, or what is his response? Then we are able to find, as the program goes on and his flights are completed, comparing his first and last flights, has there been any improvement in his performance over this exposure period to the weightless condition? Does he learn to adapt, adjust to this? Then we are evaluating the foods, type of foods which have been developed and prepared for use in a weightless condition. One type particularly, and I believe these were supplied by the Quartermasters, which -- who is that, Col Powers?

POWERS: I think the Army Quartermaster Corps.

DR. HAWKINS: The Army Quartermaster in Chicago. It is a toothpaste type of tube that maybe one of the Astronauts might be able to explain his exact impression of this. Then we are trying to evaluate, of course, how we are going to give water to this individual, what type of system is best suited to provide him with the physiological requirements that he must have every day. They have had a chance, then, to work with these items, to test them out themselves, so that they know what the problems are. Then at the same time, under such a flight, they were wearing the pressure suit.

Then we have also given them each an opportunity to actually fly some of the parabolas, fly through the weightless condition themselves, again giving them a chance to actually measure their own responses and reaction times during the weightless condition under a characteristic flight operation. I think really that's about the essence of the program, gentlemen, unless you have specific questions you wisk to ask.

POWERS: The four flights, then, to run down very quickly, are, one: in a normal flying suit; the second flight in a Mark 4 pressure suit; the third with the Astronaut flying parabolas from the rear seat themselves; and the fourth, a repeat of the first flight to find out whether or not the Astronauts may have acquired some information and training and capabilities as a result of the earlier flights. O.k. Now, I have talked too much; the floor is now open for questions and answers. Mr. Miles?

QUESTION: Can you tell us what you have found in the way of effects and reaction times and improvements in the four flights?

DR. HAWKINS: Mr. Miles, no, I cannot, under the flight -- er, under the study that has been conducted here, because the time schedule that we have been working on has been a very critical one. We have been flying three flights a day, instrumented, fully instrumented flights, and this is something which is just, well, just not usually done. It's an extremely tedious task that requires a lot of support and effort to try to stop and evaluate all the data that you are collecting at the time. It is just impossible. As I say, we are not yet finished. We have a couple more days to go, and only then will we be able to sit down, really, and evaluate and run correlation curves on all this to give you the type of answer that you want.

QUESTION: Can you tell us what physiological read-outs you are getting on your telemetry here?

DR. HAWKINS: Yes, sir, this is -- we are measuring, well, actually we are recording three leads, EKG's, electrocardiograms, the lead one, two, and three, which are standard leads, and then we are measuring respiration, and we are measuring blood pressure. Simultaneously with this, of course, we are measuring the accelerations in three axes that are acting upon the individual, and this is all simultaneously recorded so that we know exactly when we are in zero gravity and when we are not.

QUESTION: At what altitudes are you making these runs?

DR. HAWKINS: If I may refer this question to Captain Breding, who is the pilot from the school, he can best answer this. He is the expert in maneuvers on this.

POWERS: Reddy, why don't you come on up and fly one of these things with your hands, if you will, and maybe -- I know I had to find out that way.

BREDING: I don't have powered hands!

CARPENTER: I'd better get out of the way in case you get out of control in maneuvers there! (Laughter)

POWERS: All right, talk us through a flight.

BREDING: Well, like Dr. Hawkins said, our missions run about 50, 60 minutes approximately. This is from take-off until landing. During this period of time we have accomplished three to four parabolic curves and have returned and landed. After take-off -- well, prior to take-off, we have our count-down to go through, whatever you want to call it, to get all the equipment in order before we move out to the end of the runway. We take off and climb, and during the climb we are also checking equipment to insure that it is working with the ground station. We climb to an altitude of 40,000 feet and attain at that time the highest mach that we can conservatively allow, that is, saving our fuel and time. From this point we go into a gradual push-over or dive, the purpose of which is to again increase our mach without losing too much altitude. When we have reached an altitude of 25 to 20,000 feet in a dive going on down at a 30 degree angle of dive, we reach 25 to 20 thousand feet, somewhere in that area, 5,000 foot area, we rotate the nose upward. At this point we will have the highest mach that we can attain in the altitude we have used, and we will rotate the nose upward to an angle of attack of near 55 degrees read from the cockpit -- (interrupted).

SHEPARD: Attitude.

BREDING: Yes, angle of attitude, with the nose up. Now, at this point we -- what we say in G.I. language, "stick it" in head to toe zero G force, and when we do, we reduce our power and control it fore and aft zero G, so what we are after at this point, right at this point there are no forces at all, neither upward, downward, sideways, fore, and aft. To assist us to exact this thing, plus our instruments, we use the old millionaire's golf ball -- (laughter) -- and we keep that thing floating right -- we have it on a string hanging in the cockpit, we keep that thing floating; we are neither pulling it nor crowding it either way. We then know we are at a weightless state. To attain this is a job.

Once we get in it, it isn't so hard to hold it, depending on the ambient air, the engine in the airplane, the mach we enter, will take us -- will crest us out on top at various altitudes. More than likely it will be above 35,000 feet. We have come out over the top, a good one up to 45,000. Then we are at our slowest airspeed which we can allow for good engine operation. We get our airspeed too low, and still operating our engine, will experience an engine compressor stall, and this is what we avoid; so we have to go over the top at a certain predetermined airspeed. This is usually no lower than 160 knots.

We are still in zero gravity at this point. We rotate the nose over the top; we come down the back slope, the back side, and holding weightlessness, the weightless state yet, and at that point we are gaining, and we must gain airspeed rapidly. We have to manipulate our power to keep up with the golf ball until we again come to 20,000 feet. At this time we start initially our pull-out, and the angle at the backside will, of course, not exceed vertical, but at times it will almost reach it in an effort to maintain the end weightlessness.

BREDING: : Many things we experience. Each flight is a different flight, but to finish up the curve now and reach our 20,000 foot point, we initiate a pull-out and we do it without excess G forces on the airplane and the engine. We pull it out at not over, if we can avoid it, a 3 G maneuver pull-out, and from this point we will climb up to another parabolic should the maneuver call for this. Some of our maneuvers will call for a 3 G accelerated past weightlessness maneuver. From this point we go from zero to 3 G and hold 3 G for one minute or however long we are asked to do it, and now for this program we are holding it for a minute. This gives the number two occupant the time to evaluate the difference between weightlessness and positive G force. That's what we are trying to simulate here and to measure.

POWERS: And the positive G force coming immediately after zero.

BREDING: Immediately after zero. We are in a zero state and right into 3 G force load, and we hold this. During this time the number 2 seat occupant, the Astronaut in this case, has a task to do, either feeding or working the psychomotor unit, or in one flight he will fly it himself. Now, the safety measures involved in this, number one, we require chase to cover us, that is, to keep the range we are flying in clear of other aircraft. Of course, the pilot is on instruments, he cannot do contact flying when he is flying this maneuver. We have three pilots involved from the school, Lt Wagner and Lt Streicher, who is with me now, and myself, that are primarily flying the problem.

POWERS: Thanks very much.

SHEPARD: They're real pros, too! (Laughter)

QUESTION: These three weeks of tests, have they all been in the F-100?

BREDING: : Oh, yes.

QUESTION: And is there any particular significance that you are only striving at this time for 50 to 60 second periods of weightlessness?

BREDING: : Well, this is the limitation of the aircraft that we have. I mean, we can get -- we are interested in 60 seconds now. There are times, of course, we may exceed this, but when we do, we are reaching the limitations of the power available and the vehicle in which we are operating. Incidentally, we feel it is about the best one, of course, that's available for us.

QUESTION: Isn't that about the longest periods that you have been able to attain?

BREDING: : Oh, we've overshot it some, but it is the longest period that ever has been attained, yes, by almost twice.

POWERS: Yes.

QUESTION: What would you say your maximum has been in your best run?

BREDING: Well, we don't have this but we have clocked it and we think that we can get 65 seconds.

QUESTION: What has been the normal average pull on the vessel obtainable?

BREDING: Well, it seems to me we are -- not very long ago, a couple years ago, 35 to 40 seconds was a real peak load. This is with other type aircraft.

QUESTION: Pardon my ignorance for the question, but are you talking now about the individual or the airplane?

BREDING: The airplane. These are F-100F's.

QUESTION: Now, can you graduate to a faster aircraft and get a longer period of weightlessness?

BREDING: At the present there is no other aircraft that can do it. There's -- there may be a 104 that might get 5 to 10 seconds greater, but the efforts required to fit this airplane for this kind of work overshadows the -- the requirement, for just 10 seconds. Even then this is just a calculated approach to it. They feel it should do it because of its higher mach number attained at a certain altitude.

POWERS: I suspect Captain Jordan in his F-104 probably could do more than a minute, but I doubt if we could manage to fly many flights to a hundred and some odd thousand feet to get the training.

BREDING: We are interested in weightlessness now, no forces at all in any direction. And then we multiply the problem. When we think of the total weightlessness, that's a different thing.

QUESTION: Major, may I ask, would you say that this is the most thorough study up to date in getting G reactions?

DR. HAWKINS: The most thorough?

QUESTION: Yes.

DR. HAWKINS: Yes, sir, I would, and although a lot of work has been done by people ahead of me in this game back at the school two years ago -- in fact, actually the work started back in the school as far back as 1953 -- but I think today that this is the most comprehensive effort that has been put forth. When you stop and realize the amount of data that has been collected during this period of time -- (interrupted)

QUESTION: Major, could you tell us a little bit of how this material is gathered?

DR. HAWKINS: Well, we have on board recording facilities, which is an oscillograph. At the same time we have a telemeter transmitter which we are able to transmit the same information or portions of it, whatever we want, to a telemetry receiving station, and we can get direct read-outs of this same information, so we have two facilities to rely upon for reporting this information.

QUESTION: With apologies to Mr. Miles and Mr. Wainwright, could I ask you a couple of elementary questions? When did this program, this Astronaut program actually -- the date -- not the program, actually, but the date?

POWERS: How about you answering this? (indicating Shepard)

SHEPARD: I think the training program started on the 9th of April of this year, 1959, when we had our first press conference and our first training step was to meet the press in Washington.

QUESTION: You are back where you started from! (Laughter)

SHEPARD: Back where we started from the first time. Immediately thereafter we moved families and so on the latter part of April, and that's where we really got together in Langley Field using the NASA facilities there as our headquarters, and started up training at that time. It has been a continual process, step by step process.

QUESTION: Who would I direct a question to when the -- well, when the one of the seven actually participates in the program?

POWERS: You mean, when are they going to fly?

QUESTION: Well, that's the question.

POWERS: The only thing we have set as far as the timetable is concerned is during the calendar year 1961 we expect to send an Astronaut on a manned orbital flight.

QUESTION: Who would I direct another question to -- does anyone have the opinion that Russia has actually attempted this same thing without success in recent months?

POWERS: We don't have any more information, I don't think, than you or Mr. Miles or any of your associates here. We watch your reporting very closely and we think you are probably familiar with the controversy that has been going on the last two or three days, where there was a report out of Rome that the Russians have tried and lost several astronauts. The Russians on an official basis have denied this. Earlier there have been reports that they had a program for training astronauts; later when Mr. Sedov came over to the United States and actually visited Langley Field, he denied that they had any such programs, so I can't answer your question. I don't know.

SHEPARD: For information just a little bit, since you mentioned Sedov, later on at a meeting of the American Rocket Society in Washington, he was talking to Von Braun, and one of the points he made clear to him at that time was that they were essentially adopting the same philosophy as we were, the philosophy that they would not send a man into space until they had the feeling they could bring him back successfully. Apparently if they are following this philosophy they are shooting for a reliability factor of 90 out of a 100 before they go. Quite frankly, we are not in that business, we are in the business of putting hardware in the sky, and we can only guess, as you people are doing.

QUESTION: Colonel, again another elementary question. Who would make the final decision which one of the seven, Mr. Carpenter, Mr. Shepard, or the other five would go?

POWERS: I suspect in the final analysis Mr. Robert Gilruth is the director of Project Mercury for NASA; it will be his responsibility to at least recommend the selection. I feel that in view of the importance of the program to the nation, that the selection will probably be confirmed at a very high level of government.

QUESTION: May I go back to a local level for a minute? Could you tell me what your reactions have been to these -- to these zero G tests? I know you have been through zero G before as pilots. Have you ever been through a period this long and what has your general reactions been on the food attempts and the gizmo --

CARPENTER: O.k., I will start with the gizmo. I haven't eaten yet; Al has. I will let him talk about eating. It doesn't appear to be any more difficult in the weightless condition than it is in the 1 G condition. This is just my personal impression. We don't have access to the data yet, either, but it doesn't seem to be a problem so far.

QUESTION: Is there any particular feeling? Early in this Mercury program, I have been told by pilots before that zero G sometimes makes a man sick, sometimes makes him feel light-headed; is there any particular physiological feeling that you have in your zero G to this extent of time?

CARPENTER: None as far as I am concerned. Some people get seasick, too, but everyone doesn't. I think there are probably some that zero G might make sick. It didn't happen to make me sick. It's unnoticeable; you can -- if you are given a task that takes, say, 25% of your concentration, this is enough to make you completely unaware of the weightless condition.

QUESTION: Has that been your feeling, too? (Indicating Shepard)

SHEPARD: Yes, qualitatively speaking for the moment, because we haven't seen the day yet, either, it is a very comfortable sensation, and there is no obvious restriction to any of the movements that you make under 1 G conditions as opposed to those under zero G conditions. I think, maybe to clarify your question about the uneasy feeling, this, to my knowledge at least, has occurred primarily in the C-131 at Wright-Pat, where the parabola is the same essentially, but it's flown in a little more abrupt manner. Now, the 100 has better performance and so it goes through a longer parabola, it's a higher G airplane, so you can do more things more smoothly and abruptly -- er, more smoothly and gradually, whereas the 131 is kind of an abrupt thing, and I think most of the difficulty most people have had there, though none of us had it, is the continual slamming back into the floor of the cabin after they finished the zero G trajectory. They bounce around quite a bit; they were not tied down as we are here in the 100's, and I think this may clarify that point.

QUESTION: How about the eats?

SHEPARD: Terrific. I had chopped beef this morning out of a toothpaste tube, and it was very tasty. As a matter of fact, I had eaten quite a bit of breakfast and thought these guys might have given me something that wasn't quite so tasty, but I ate more than my share.

CARPENTER: He ate what was supposed to be mine! (Laughter)

SHEPARD: I ate what was supposed to be his.

QUESTION: Was there any difficulty swallowing?

SHEPARD: No, apparently -- of course, we have not worked with any large chunks of food. We are working with several different sizes or grains of baby food, if you want to call it that to refer to it for comparative sizes, and you squeeze it in your mouth, you can either squeeze or suck or both, depending on how hungry you are and what rate you want to take it in, and once you get it inside and get the muscles working, then it is purely a muscular movement from there on down. Same thing with the water, the water bottle that I used. We used two, actually; this one we are experimenting with, and a catsup bottle. The catsup bottle was a little more successful. To just take the water in, you can squirt up or down or sideways, it really makes no difference as long as you get the nozzle in the general direction of your mouth.

QUESTION: You put the bottle in your mouth or -- ?

SHEPARD: You can do either, or squirt it, because when you squirt it goes straight out. There is no parabola trajectory, so when you squirt it it goes right straight out, and once you get the aiming point you're all set. Here again, the muscular action takes it right on down.

QUESTION: Apparently automatic, then, the actual swallowing process?

SHEPARD: Yes, you notice no difference. It is not uncomfortable; you notice no uneasy feeling at all in getting it down in your tummy.

QUESTION: You don't do this with a pressure suit on, though, do you?

SHEPARD: Well, we actually did it with the suit on, but not inflated. The face plate was opened and we took it through the face plate. Now, there is another means of doing it, of course, if you want to provide a pressure lock in the face plate. This has been tried before; also with a plastic tube that will come in the face plate. With a fairly small hole you can get the tube in there and maintain the pressure ceiling and actually there you squeeze and suck to get it in.

POWERS: Actually the conditions we are duplicating here are the same that will be encountered in the normal Project Mercury capsule flight; assuming that the capsule life support system is operational, the man will ride unpressurized as far as his suit is concerned with the face plate open, normal standard procedure.

QUESTION: The first real experience, though, I mean extended experience, with weightlessness will be the original flight?

CARPENTER: For us.

QUESTION: And how long do you feel you will be weightless?

SHEPARD: Five and a half minutes.

QUESTION: Five and a half minutes?

SHEPARD: Yes. This is a -- here again, if you ask us in a qualitative analysis of weightlessness, it might be anything between plus or minus .05 G. As far as the gauges are concerned, I think they are shooting for about .02 G as a criteria.

QUESTION: I would like to ask the doctor one more question, if I may. Major, what relationship do you feel there is to a minute of weightlessness to fifteen minutes? Do you think that you can get a proper analysis of the reactions? How do you figure on extrapolating the extension of this for fifteen minutes or five minutes or four and a half hours?

DR. HAWKINS: Well, the only -- of course, the final answer to what the prolonged effect of weightlessness will be will only be known when we have a man in orbit for extended periods of time of hours or days. Now, then, I feel like that within a short period of one minute that this is long enough for the body's normal responses to make an adjustment to the new condition under which it finds itself, that we can evaluate what changes will probably occur, and from this, then -- of course, we will have to do our best to extrapolate, but again the final answer, we realize, as to prolonged duration of days will only come after the man is actually in this orbit.

POWERS: I would like to point out, just one other thing, that we are acquiring in connection with Project Mercury as a result of this flight operation, since we are transmitting telemetry back to a telemetry center, we are acquiring experience on the part of our flight surgeon and other medical personnel in watching that telemetry and observing the data. We have tried on a couple of these -- well, every man has tried to talk, for example, under weightlessness, and we are using the standard or what is becoming standard Project Mercury pilot report transmissions. These, again, are coming down into the telemetry center and this is all being recorded on tape so that we can bring it back to NASA to our space task group headquarters at Langley and use the tapes that we have acquired here now to train all the people who will be involved in monitoring the Mercury astronaut flights in the Mercury capsule, so we are -- this is kind of a triple or quadruple-edged program. The School of Aviation Medicine is acquiring data that they would like to have very much, and they are getting it. Our astronauts are flying under zero G conditions to familiarize themselves with these conditions. We are acquiring data for our own purposes and we are acquiring base material that we can take back now to our management organization at Langley to train other people in the techniques of monitoring the entire system.

QUESTION: Are you concluding now your zero gravity training phase or will you still have an advanced phase that you will come back here for?

POWERS: Well, this week winds up this phase of the training, and unless Dr. Hawkins and his people can come up with a new airplane that will give us something in the order of 3 or 4 or 5 times as much weightlessness as we are now getting, I suspect our next experience will be on the tip of a Redstone booster. Right?

CARPENTER: That's correct.

QUESTION: What will be the next test for these fellows before that flight? Do you have any more set up in your program, shall we say experiences; what would be next?

POWERS: Not at zero G.

QUESTION: No, but what would be the next phase of their training, shall we say?

POWERS: Well, for the next several months I think we are going to be involved in working out the pressure suit business, are we not?

SHEPARD: That's correct. We have had some training on the centrifuge machines, which, if you are familiar with it, is the oversize cream separator that flings you around the room, that sort of thing. We have had some of that in flight here, as Shorty pointed out. We are most anxious to try the combination of our pressure suits and the couch which we will be using in the capsule and which is being and has been and is being molded for us now by McDonnell at St. Louis, and the centrifuge which will be configured for an ambient air pressure equal to 27,000 feet, which is our flight environment, 5 psi absolute.

QUESTION: So far as your tests in the centrifuge have been in the normal flight position and not on the couch, then?

SHEPARD: No, in a reclining position.

QUESTION: It has been on a couch?

SHEPARD: That's right, but the couch was molded with just this type of suit on (indicating orange-colored flying suit being worn by himself), but the pressure suit is larger so we need a different type couch. They're fiberglass molds. That's the first step. We have training profiles to run in simulating machines where you actually provide a pilot input on the hand controller, side arm hand controller, around three axes and your input is matched through a computing system against the known aerodynamic parameters and moment of inertial parameters of the capsule, various stages of flight. It is similar to -- well, it is a glorified link trainer, it is a glorified operational trainer as we know it now. We have fed these things into a computer. We have more to learn about the capsule as such, and then we are going to be taking a very close part in the mating of the capsule and the Redstone, and, of course, finally the flight itself.

QUESTION: What controls will you have in flight with the capsule?

SHEPARD: The primary means of controlling the capsule around its three axes of roll, pitch, and yaw are through the use of hydrogen peroxide jets which are located in such a position on the outside of the capsule to fire with a couple, a rolling couple, a pitching couple, or a yawing couple; this is not a pure single jet.

QUESTION: Um-hum --

SHEPARD: And these are controlled either through an automatic auto-pilot, through a fly-by wire system, or straight mechanical tie-in from the hand control.

QUESTION: Is this hand control similar to the one we know in the X-15?

SHEPARD: It is similar. Our controls run three axes, and their controls run two.

POWERS: A good bit of the Astronauts' time in the next few months will be absorbed in the process of making engineering contributions to the final delivery of the systems we are going to use. Each Astronaut has been assigned to an area -- subject area of specialization. Alan Shepard's area is the recovery process. Scott Carpenter's is navigation and navigational aids. In these specialized areas they are members of working panels composed of NASA people and representatives of the other agencies that are involved in the particular system under study, and they are, as engineers, actually making a contribution to the development and production of these systems, and this will be -- as these items now come down toward the delivery line, delivery point, this, I think, will become more time-consuming than it has been in the past.

QUESTION: After one man has been picked or does go into this orbit, orbital program, what will be the future of the other six men then?

SHEPARD: We all fall on our swords! (Laughter)

QUESTION: Will there be future programmed flights for these men, or what actually is the feeling there?

POWERS: The only thing we have programmed right now is Project Mercury. Now, this is not to say that we aren't looking down into the future into more sophisticated systems, larger space vehicles, deep space penetrations. As far as the future of the seven men are concerned, I don't think they know any more about their future than I do. You might comment on that if you like, Scott.

SHEPARD: It looks like we will all get a ride in Mercury, I mean.

CARPENTER: You can fall on your sword; I am going to stick around for the second try! (Laughter) It looks like we'll all get a ride within the confines of Mercury, either a Redstone or an Atlas ride, and I think it's pretty safe to say that, if they will have us, that we will be in the space business from here on out in one form or another.

POWERS: I think it would be a great waste. Actually, what we are doing again while we are conducting this program is creating another, an additional national resource in the form of seven skilled engineer-pilots who will have space flight experience.

SHEPARD: Next step is an instructor's role! (Laughter)

QUESTION: Back to the desk, huh?

QUESTION: Will there be another occasion for the Astronauts to return to Edwards for any type of tests, or Southern California, I will put it that way.

POWERS: We don't have anything programmed that I know of.

CARPENTER: We have nothing scheduled at the moment.

QUESTION: We are seeing the last of you, then?

CARPENTER: This I will have to say is problematical. (Laughter)

QUESTION: Well, I'm sorry, I should have reworded that. On the subject of speaking, is it difficult to speak clearly or make yourselves understood in zero G?

SHEPARD: No. I had that flight today where I transmitted a passage of -- not from Shakespeare -- a typical flight report at 1 G and then zero G, and there is no noticeable difference. There may be some difference in the rich, bell-like tones as they are received on the ground, but certainly I had no trouble. (Laughter)

QUESTION: They had no difficulty in reading you from the ground?

SHEPARD: No, apparently not.

QUESTION: You say you transmitted a passage. You mean you were reading it?

SHEPARD: Yes, what might be a flight report to one of the tracking stations as you went over in orbital flight as to altitude, fuel conditions, conditions of the interior of the cabin, and that sort of thing.

QUESTION: Another elementary question, colonel: Where is the Mercury scheduled to emanate from on the ground?

POWERS: The launch of the orbital missions, well, the launch of all of the missions will be from Cape Canaveral, Florida, and our recovery area is off the Atlantic Missile Range in the South Atlantic.

QUESTION: Is General Yates going to let the press in?

POWERS: You would have to ask General Yates on that.

SHEPARD: In the nose cone? (Laughter)

QUESTION: One more question. How far and how high will the Redstone missile take you?

CARPENTER: 105 miles up and 250 out, approximately.

POWERS: These are approximations. I think you would be safe in rounding it off at about 100 miles.

QUESTION: Scott, have you done any navigational problems in the weightless condition?

CARPENTER: No, only the ones we have talked about here, which doesn't include navigation, but it includes mental tasks. There is no reason to believe that a navigational problem would be any more difficult than what we have been doing here.

QUESTION: What has been the actual experiences in the centrifuge, then, compared to this? What tasks have you performed there?

CARPENTER: Control tasks, and they are considerably more difficult than this, and the effects of increased acceleration are much harder to live with than the lack of gravity.

SHEPARD: It's a real pleasure, actually, to go the other way for a change after fighting the centrifuge until it whips you or you whip it, and come out here and actually feel comfortable doing the investigations.

QUESTION: How high did you go in the centrifuge, how many G's?

SHEPARD: As a group we went between 14 and 16; I think some went to 18 G's.

POWERS: Unless there are other questions, we have now been at it for about an hour and forty minutes. On behalf of NASA we want to thank you for coming, and hope we have been responsive to your inquiries. When we get back to the west coast, and I am sure we will come back one way or another -- we kind of like it out here -- we look forward to seeing all of you again. Thank you very much.

(Thereupon, the Press Conference was concluded).

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

NASA RELEASE NO. 59-278
DU. 2-6325

For Immediate Release
December 16, 1959

VANGUARD III TRANSMITTERS NOW SILENT

The tracking and telemetry transmitters within the Vanguard III satellite became silent last weekend, according to NASA scientists. Launched September 18, the Vanguard III transmitted tracking signals and scientific data over a period of 85 days. The satellite's silver-zinc batteries had a nominal lifetime of 90 days.

The Vanguard III, the last experiment in the Vanguard series, was instrumented to measure the earth's magnetic field, X-rays from the sun, and environmental conditions in space. Shaped like an over-sized ice cream cone -- a 26-inch tapered tube extending from a 20-inch sphere -- the satellite was fabricated of magnesium and fiberglass. The instrumented payload weighs 50 pounds; the attached third-stage casing weighs an additional 50 pounds.

During its transmitting lifetime, Vanguard III traveled approximately 30,874,000 miles in its 938 orbits around the earth. The minitrack station at Santiago, Chile, was the last to interrogate the satellite at 9:01 a.m., EST, December 11. The last tracking report came from Woomera, Australia, which picked up signals the same day at 5:39 p.m., EST.

The now silent satellite is expected to remain in orbit up to 40 years.

Telemetry records from the satellite are still in the early stages of processing and analysis. However, scientists connected with the experiment report the quality and quantity of data received have been excellent.

- END -

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

NASA RELEASE NO. 59-281
DU. 2-6325

For Release:
10:15 a.m.
December 17, 1959

CAPE CANAVERAL, FLORIDA, DECEMBER 17, 1959 -- The launching of a Thor-Able space probe has been postponed because of electronic problems which arose in final checkout of the instrumented payload. The launching was to boost a 90-pound payload designed to explore space between earth and Venus.

The scientific package contains a UHF 150-watt transmitter designed to relay information from many million miles out in space. The 26-inch spheroid with four solar cell "paddlewheels" jutting from its equator, is instrumented to provide micrometeorite, radiation, magnetic field and temperature information.

A new firing date for this probe has not been scheduled.

- END -

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

For Release Upon Delivery
12 noon
December 17, 1959

SPACE -- PAST, PRESENT, AND FUTURE

by

Dr. Homer J. Stewart
Director, Office of Program Planning and Evaluation
National Aeronautics and Space Administration

SPACE -- PAST, PRESENT, AND FUTURE

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Dr. Homer J. Stewart
Director, Office of Program Planning and Evaluation
National Aeronautics and Space Administration

For Presentation to the
American Ordnance Association, Washington Post
Willard Hotel, Washington, D. C.
December 17, 1959

I am very happy to be accorded the privilege of addressing the Washington Post of the American Ordnance Association on this occasion. I have for many years followed AOA activities and know of your concern for and your efforts on behalf of the security of the United States -- not only in the special line of military defense, but also in the broader context of education, the economic well-being of the country, and international affairs.

At Christmas time, particularly, the well-being of all mankind seems to be the concern of all. In government operations, it is also a period of introspection and re-examination of policies and programs brought on by impending Congressional hearings aimed at determining in detail what our national activities should be in the new year. Accordingly, I propose to re-examine briefly the national activities in space as I see them, not only in the past and the present, but also as they reflect an image of the future.

It is obvious that the last decade has produced dramatic changes in our national viewpoints on the question of the exploration of space. I would like to refer back, as a starting point, to a series of tests which were performed in 1949 under the sponsorship of the Army Ordnance Department under the code name, "Bumper." This project was carried out jointly by the Jet Propulsion Laboratory, the General Electric Company, and the Douglas Aircraft Company. The object of the project was to launch a Wac Corporal upper-stage vehicle from a V-2, and demonstrate a prototype high-performance, multi-stage rocket vehicle system. As you may recall, the project was entirely successful in demonstrating the feasibility of several unproved concepts: the staging of rocket vehicles at high altitudes, the utility of ablative materials for protection from aerodynamic heating, and a method of stabilizing rocket propelled vehicles operating in the vacuum of space. One of the most significant interpretations of this test to the engineers familiar with the work was the fact that the test showed that there were no longer any purely scientific barriers to the construction of vehicles capable of space flight. The barrier was engineering.

In the general atmosphere prevailing at that time, this most significant result could not be announced from the house tops. Indeed, public discussion of space activities was considered at that time to be in rather poor taste. It was only within the privacy of some scientific and engineering institutions, such as Rand or the Jet Propulsion Laboratory, that the significance of space was dimly recognized. I say "dimly," even though JPL, working for the Navy, and Rand, for the Air Force, had already made studies of satellite problems.

Within the United States the first definite step toward the active exploration of space occurred in the summer of 1955 when the President announced that we would, as part of our participation in the International Geophysical Year activity, attempt to launch at least one scientifically instrumented satellite into an orbit around the earth during the 1957-1958 period. In our planning to carry out this commitment we decided to do the work on the smallest significant scale, more as a demonstration of the feasibility of space activities than as a real beginning of the exploration of space.

The next critical step in inaugurating a national program for the exploration of space came three years later with the passage of the National Aeronautics and Space Act of 1958. The Act established the National Aeronautics and Space Administration and, as a matter of national policy, directed NASA to "plan, direct, and conduct" such "activities as may be required for the exploration of space." It is this instruction to carry out the exploration of space which is the unique feature of the charter of NASA, and it is this responsibility which requires NASA to make significant contributions over the long term to the security of the nation -- in a new way.

The long-term security of our country requires that we retain the climate of intellectual vigor and vitality, which in the past has been such an important element of our national strength -- a factor that has produced and has attracted the best efforts of mankind everywhere. The dimly felt but undeniably important long-term results of the exploration of space must be expected to affect strongly the intellectual life of all mankind, and we in the United States must play a responsible role in exploring this new frontier for the benefit of humanity.

We have by now carried out a number of experiments which have clearly demonstrated the feasibility of operations in space. Our scientific results from these experiments have convinced us that space is an intellectually fruitful environment for scientific experiment. For example, Explorer I, whose successful launching in January 1958 formally fulfilled our national commitment to the IGY, demonstrated that our understanding of the outer reaches of the earth's atmosphere was far from correct. As a consequence, many of our later flights have been instrumented to explore in greater detail the great radiation belts surrounding the Earth. We have also taken our first step beyond the gravitational field of the Earth with our Pioneer IV space probe, launched last March and now in orbit about the sun.

While we have had many successes and have produced much significant scientific information, our activities have not been fully satisfying because our scale of activity is small and limited in comparison with that of our only competitor, the USSR. The Soviet space exploration program, presumably, was formally organized in 1954 when their Interdepartmental

Commission for Interplanetary Communication was established -- four years before NASA was created. It is now evident that their initial planning called for an aggressive space program so that highly significant exploratory experiments could be boosted into space. In effect, they bypassed completely the small-scale feasibility demonstration -- the Vanguard phase -- which was our initial step.

At the present time our program for the exploration of space has two principal features: (1) using interim capabilities created on a short-term basis for the limited uses they permit, (2) preparation of more versatile and powerful equipments to carry us into a sound program for the long haul. Both of these activities are essential. For example, if we were to refrain from using the interim vehicles and only concentrate our entire effort on long-term preparation for the future, not only would our position in the international arena be more greatly jeopardized, but, technically, we would have probably missed some of the scientific factors which may well be of great significance in guiding the course of our future activities. On the other hand, if we were to concentrate our entire attention on the exploitation of our present limited capacities with no effort spent on preparing more efficient and more capable equipment for the future, we would be placed in an even less defensible position. Maintaining a proper balance between these two activities is consequently one of our most serious problems.

In order to serve our interim needs for space vehicles, we are using the upper-stage rockets developed under the Vanguard and Jupiter C programs. Thus our initial venture into space is dependent on these smaller rockets and the larger IRBM booster rockets developed as part of our military effort. For example, we have combined the spinning cluster of solid propellant rockets used on Explorer I with the Jupiter IRBM to create what we now call the Juno II vehicle. The upper stages of the Vanguard have been combined with the Thor IRBM to create the Thor-Able and the Thor-Delta vehicles.

The largest scientific payloads we have up to this date placed in orbit are the 90-pound Explorer VII payload, launched by a Juno II, and the 142-pound Explorer VI payload launched by a Thor-Able. Somewhat larger payloads have been launched in the Discoverer series; however, these somewhat larger payloads are possible only in very low-perigee, short-lived orbits which are not well suited for most space science experiments.

One way to compare the effectiveness of these interim configurations with more appropriately designed equipment is to note the ratio of takeoff weight to payload weight. For the 142-pound Explorer VI, the ratio is about 750 to 1. A properly proportioned three-stage vehicle using our current level of technology, the same as that used in any of our large military vehicles, would have takeoff weight to payload weight ratio of 40 or 50 to 1; that is, our present exploitation of the booster vehicle in satellite orbits is less than 10% effective. Similarly, the Pioneer IV had a net payload weight of only 12 pounds for a ratio of about 8000 to 1. Again, a properly proportioned vehicle would produce a ratio of about 150 or 200 to 1, and our efficiency of exploitation, in this case, is thus less than 3%.

These ratios show clearly that our present operations are not primarily limited by the size of our first-stage booster rockets -- even though in the long run we require substantially larger booster rockets. Our present primary limitation lies in the fact that we do not have the appropriately scaled upper-stage rockets to exploit efficiently our large military booster rockets as first-stage launching vehicles.

We are now developing the Agena and the Centaur upper-stage rockets to permit an efficient exploitation of our IRBM and ICBM boosters. For comparison it may be of interest to note that the high-energy propellant development used in Centaur is expected to produce a payload capacity in an orbit corresponding to a ratio of takeoff weight to payload weight of about 30 to 1, twenty-five times as effective an exploitation as our best effort to date.

We are also proceeding with the development of the Saturn booster which is roughly four times the size of the Atlas ICBM. Part of the Saturn project envisions the use of upper-stage rockets to exploit efficiently this very large booster capacity. Still longer-term and higher-performance vehicles are required for later space exploration missions. A first step toward satisfying this future requirement is the development of a one-and-a-half million pound thrust single chamber rocket engine which can be clustered to power the very large vehicles which we foresee for the future.

Until we have the upper-stage developments to permit us to use our present booster capacity effectively we will not be able to carry out the more significant space exploration missions which require precision guidance and substantial pay-loads. It is also quite apparent that until these new equip-ments are available the cost per pound of payload in orbit will be inordinately high. It is necessary for us to go to great lengths in miniaturization of equipment in order to maximize the effectiveness of our overall operation.

Even with these limitations we expect our flight program in this interim period, which will cover all of the next year, to produce significant and important results. We will continue our exploration of the nature of the great radiation belt and of the ultraviolet and gamma radiation outside the Earth's atmosphere. We expect to make further measurements of the infrared radiation characteristics of the Earth and related measurements of significance to the meteorological forecasting problem. We expect to make some communications experiments using a large 100-foot sphere as a passive reflector for radio transmissions. We also expect to be well down the road on the final phases of preparation for Project Mercury. The capsule check-out should be nearly completed and the Astronauts should have entered the most advanced phase of their training with the suborbital Redstone flights. It is perhaps of interest at this point to note that Project Mercury requires only a relatively low-perigee, short-lived orbit. Consequently, we do not have to wait for the development of more capable equipment; the standard Atlas vehicle is capable of producing the required performance.

In summary, we are carrying out a flight program which has yielded, and will continue to yield, interesting and significant results, even though we are still severely limited by the interim nature of our flight vehicles. We have underway the new vehicle developments which we expect will increase greatly our efficiency of operation and our capability to operate in space. The rate at which we will progress into the exploration of space is of course dependent upon the resources which are placed at our disposal; however, I can assure you that we at NASA are making every effort to make certain that these resources are efficiently used so as to produce the most effective over-all program.

Thank you and Seasons Greetings!

NASA Release No. 59-280

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

NASA RELEASE NO. 59-282
DU. 2-6325

FOR RELEASE AFTER LAUNCH
DATE December 22, 1959

A four-stage sounding rocket was launched early today from Wallops Station, Virginia, in a joint United States-Canadian experiment. The launching took place at 2:56 a.m. EST.

Purposes of the rocket launching were:

1. To measure the intensity of galactic noise, an experiment provided by the Defense Research Telecommunications Establishment of Ottawa;

2. To determine the performance of the X248 rocket in a vacuum. This experiment, as well as the rocket and launching, are part of the space sciences program of the National Aeronautics and Space Administration.

The 48-foot Javelin launching vehicle was programmed to propel the payload to an altitude of about 560 miles. Galactic noise, or the radio signals emanating in space, are absorbed by the ionosphere and it is necessary to measure them at altitudes above approximately 450 miles. The 48-pound payload contained a three megacycle radio receiver which telemetered galactic radio signals to the earth.

The X248 engine, which was the fourth stage of the Javelin, is to be the third stage of NASA's Delta launching vehicle now under development. Data on its performance also were telemetered to ground stations.

Telemetry stations for the experiment were located at Wallops, Cape Hatteras, North Carolina, and Cape Canaveral, Florida.

The launching vehicle consisted of a Honest John and two Nikes as well as the X248. Takeoff weight was about 7,000 pounds.

The payload was estimated to have impacted in the Atlantic Ocean about 600 miles from Wallops.

- END -

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

Release No. 59-284
DU. 2-6325

FOR IMMEDIATE RELEASE
December 29, 1959

NASA APPOINTS CONGRESSIONAL LIAISON OFFICER

David Keyser has been appointed Chief Congressional Liaison Officer for the National Aeronautics and Space Administration to serve under the Assistant Administrator for Congressional Relations, James P. Gleason.

Keyser was administrative assistant to Representative Charles J. Kersten of the Fifth District of Wisconsin from 1951 to 1955.

Keyser will contact members and committees of the Senate and House of Representatives on matters relating to NASA.

Prior to his NASA appointment, he worked as a municipal consultant, assisting various city governments in the East and Middle West to organize and codify city ordinances.

Keyser was born in Milwaukee, Wisc., on September 27, 1918. He attended schools in Milwaukee and graduated with a bachelor of laws degree from Marquette University in 1942.

During World War II, he served with the Army Air Corps as communications officer for a P-47 fighter squadron in the European theatre.

After the war, he practiced law in Milwaukee before joining the staff of Rep. Kersten.

Mr. and Mrs. Keyser, who is the former Jane LaBissoniere of Milwaukee, have six children and live at 7906 Sleaford Place, Bethesda, Md.

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NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

WASHINGTON 25, D. C.

Wednesday, 30 December 1959

PRESS CONFERENCE

Explorer VII
Project Background and Experiments

The press conference was called to order at 1:30 p.m., Mr. Herb Rosen presiding.

Participants:

Dr. Homer E. Newell, Jr., Assistant Director, Space Sciences, National Aeronautics and Space Administration.

Mr. Herman E. LaGow, Head, Planetary Atmospheres Branch, NASA Goddard Space Flight Center.

Dr. Verner E. Suomi, Professor of Meteorology and Soils, University of Wisconsin.

Mr. Gerhard Heller, Research Projects Laboratory, Army Ballistic Missile Agency, Redstone Arsenal, Alabama.

Martin A. Pomerantz, Director, Bartol Research Foundation of the Franklin Institute, Swarthmore, Pennsylvania.

Mr. Arthur W. Thompson, Research Projects Laboratory, Army Ballistic Missile Agency, Redstone Arsenal, Alabama.

Mr. Josef Boehm, Guidance and Control Laboratory, Army Ballistic Missile Agency, Huntsville, Alabama.

Mr. Harry Carpenter, Operations Manager of NASA's World-Wide Tracking Network.

Mr. Brian O'Brien, State University of Iowa.

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NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

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Mr. Harry Carpenter, Operations Manager of NASA's World-Wide Tracking Network.

Mr. Brian O'Brien, State University of Iowa.

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MR. ROSEN: Good afternoon, ladies and gentlemen. Let me, on behalf of all those present, NASA, wish you all a belated Merry Christmas and a forthcoming happy New Year.

The purpose for bringing you together is to bring you up to date on the results obtained from our Explorer VII scientific satellite.

Now for the ground rules. Each scientist has a short statement to make. The statements are not prepared; they are all on notes, rough notes. You have decided that you will have questions and answers after each one of the statements.

Transcripts of the proceedings will be available some time tomorrow afternoon.

There are some statements I have to make for the record as well.

This satellite we are going to talk about, Explorer VII, was designed to gather scientific data as part of the U.S. contribution to the International Geophysical Year. It was the last firing of the IGY planned series of space experiments. Therefore, it was sort of nicknamed as the IGY radiation satellite.

Just to review for you some of the things that have occurred: This is Explorer VII. It was launched by a Juno II booster on October 13, 1959, at 11:31 a.m., Eastern Daylight Time. The scientific payload weighed 92.3 pounds. As of November 28th, the perigee was 346 miles; the apogee, 673 miles; its period is 101.32 minutes; velocity at perigee, 17,274 miles per hour; velocity at apogee, 16,049 miles per hour.

As I said, each of the scientists here will review the experiments that they were responsible for and I would like to list for you the companies and the experiments that were involved. Outside on the table there are copies of this project background and experiments.

ABMA was responsible for the packaging, testing, and temperature measurements. NASA was responsible for the micrometeorite experiment. The State University of Iowa, the radiation experiment. The University of Wisconsin, heat balance experiment. The Naval Research Laboratory, the Lyman-alpha X-ray experiment. Bartol Research Foundation of Franklin Institute, and Research Institute for Advanced

Studies of Martin Company were responsible for the heavy cosmic ray experiment. The Army Signal Corps provided the solar cell and power rings. Bulova Watch Company, the radio transmitter timer. Hoffman Electronics Corporation has manufactured the solar cells.

Here are the participants. Dr. Homer E. Newell, in the center, will be your moderator. He is the assistant director for space sciences of NASA Office of Space Flight Development.

Let's see if I can tie names and faces together. From my right, Harry Carpenter, operations manager of NASA's world'wide tracking network.

Dr. Martin A. Pomerantz, director of the Bartol Research Foundation of the Franklin Institute, Swarthmore, Pennsylvania.

I know the next one is Brian O'Brien -- they have left your name off of here, or I can't find it. They have. This is Brian O'Brien of the State University of Iowa. He is a native of Australia. He is one of Dr. van Allen's assistant professors at the University.

Then we have Herman E. LaGow, who heads the Planetary Atmospheres Branch, NASA Goddard Space Flight Center.

Next is Dr. Newell.

Then we have Arthur W. Thompson, Research Projects Laboratory, ABMA, Redstone Arsenal, Alabama.

Mr. Josef Boehn, Guidance and Control Laboratory, Army Ballistic Missile Agency, Huntsville, Alabama.

Mr. Gerhard Heller, Research Projects Laboratory, ABMA, Redstone Arsenal, Alabama.

Then Dr. Verner E. Suomi, Professor of Meteorology and soils, University of Wisconsin.

Now, with respect to the order of presentation, we will first have trapped radiation belts by Mr. O'Brien, meteorological data by Dr. Suomi, temperature control information by Mr. Heller, cosmic ray experiments by Mr. Pomerantz, the solar radiation experiment being read by Mr. LaGow who will also talk on the micrometeorite erosion, Mr. Thompson on the solar cell experiment, and Mr. Boehn on the satellite bearing.

I will turn the proceedings over to Dr. Homer E. Newell.

MR. NEWELL: Thank you, Herb.

On behalf of Space Sciences, let me add my welcome to Herb Rosen's.

The satellite we are talking about is the one that for sometime now has been known as the heavy IGY satellite, or composite radiation satellite. As you undoubtedly are well aware, and as will become more apparent during the discussion this afternoon, this satellite, like all others, is the result of a team effort and such a team needs an able project manager to keep it working, keep things tied together. The project manager in this case is Herman LaGow, and I am going to ask him to pick up the discussion and follow through with it.

Herman LaGow.

MR. LaGOW: Thank you, Dr. Newell.

Explorer VII was planned by the U.S. National Committee for the IGY, the technical panel for the earth satellite program under the chairmanship of Dr. Richard Porter. It was designed, constructed, and launched through the efforts of scientists and engineers from many organizations over the country.

This satellite is in a very stable orbit and is working in a very satisfactory manner. It is a very complex assembly and most significant in having a very long life, with an active satellite transmitting scientific data. Already magnetic tape telemetering records from the 20 megacycle transmitter total over 300 miles in length.

Today you will hear a progress report from the experimenters and engineers responsible for this satellite. First I would like to call on Dr. Brian O'Brien.

MR. O'BRIEN: The physics department of the State University of Iowa has two Geiger counters. Broadly speaking, these are designed to study the radiation, cosmic rays and radiation which doesn't fall easily into either of these categories. I may state that the apparatus is working perfectly as of now.

For the convenience of this, making two will separate the research into studies of two types of phenomena, one of these are called long-term effects and the other short-term effects. By long-term effects I mean variations, for example, in the intensity or the position of the trapped radiation zones over metric months. Since we hope that the apparatus will operate until the transmitter pulse switches off in about a year, the long life of Explorer VII is giving us an excellent opportunity for studying these long-term effects. Equally so, since they are long-term effects, I can't report any of them to you at present.

In the area of short-term effects we included, for example, the effect on the outer radiation zone of the geomagnetic storm. Although each short-term effect can be studied as a single entity, it is, of course, better in general to study several examples of each type. The expected long life of Explorer VII is also an advantage here since we are hopeful of getting several examples of these relatively rare phenomenon.

I will discuss three particular short-term effects that we have observed to date. I do want to emphasize that in two of these cases we have so far only found one example of each. On several occasions the apparatus has detected what appear to be bursts of sporadic radiation near the inner edge of the outer radiation belt. These bursts may be related to the bursts of X-rays which are observed at balloon altitudes, but at present we can only say that the cause is unknown or uncertain.

Another thing we have found from a study of the sequence of passes over North America from the 16th of October through to the 20th was an effect which apparently is related to a geomagnetic storm which began on the 18th of October. On the 18th of October only, the counters measured radiation which appears to have been generated about 20,000 kilometers -- let's call that 13,000 miles -- out from the center of the earth and that is between the two radiation belts. This phenomenon is being compared with results from Explorer IV, in which geomagnetic storms during 1958 were being studied.

The third phenomenon I want to mention is associated with what we call the Forbush phenomena. On occasions over many years people with cosmic ray detectors on the ground have observed a sudden decrease in cosmic ray intensity, generally only of a few percent and then this gradually covers over a period of several days. Quite often this Forbush decrease is associated with the geomagnetic storm.

Now, the sea-level detectors only study very high-energy cosmic rays. Recently Doctors MacDonald and Weber, from outside Iowa, have flown balloons to study the effect of the Forbush decrease on somewhat lower energy cosmic radiation and they have found that the effect is much stronger for these lower energy particles than it is for the high energy ones they have observed at sea level.

Now, with Explorer VII we can study even lower energy particles. We studied one sequence using data provided by Dr. Hugh Carmichael in Canada. He has sea level measurements and he has found for one particular Forbush decrease a nine percent change in his counting rate over a period of many days, whereas Explorer VII has found something like two to three hundred percent change. If I might round that off, the Explorer VII insofar as we can measure lower energies still than the balloon observations, we are pushing this study to even more interesting ranges.

I want to conclude with a summary which will indicate that the State University of Iowa apparatus was designed using results obtained with our apparatus on previous Explorers, three previous satellites, and also using results of other people and the results from this present satellite will be used in conjunction with previous results as a further step towards the final understanding or near final understanding of these phenomena. Explorer VII is just fitting a few more parts into the jigsaw puzzle.

Thank you.

MR. ROSEN: Are there any questions?

QUESTION: You say this is a 200 to 300 decrease, sir?

MR. O'BRIEN: The Forebusch decrease; we have the steady cosmic rays and then there is a decrease in intensity and then it gradually recovers over a period of many days, while the magnitude of that recovery is 200 or 300 percent on the intensity at the bottom of the decrease.

QUESTION: Would you draw us a curve of what you are talking about?

MR. O'BRIEN: Yes.

I am just going to draw counting rates up there, and I am going to draw time across here; then the normal cosmic ray intensity would come along here somewhere, then there is a Forbusch decrease which in the sealevel measurements is only a few percent and then this gradually recovers over a period of many days until it comes up about to that level again.

Now, in the satellite, this is all purely results which were discovered about half a day before I left Iowa, so they are rather preliminary. The decrease is more pronounced and the recovery is also more pronounced. You have probably got some effect like this in which if that is 1, this is 2 to 3.

QUESTION: So, you are getting a 200 to 300 percent decrease in intensity at that period?

MR. O'BRIEN: Yes.

QUESTION: Why? Do you have any theories?

MR. O'BRIEN: I told you, I mentioned that it only came out half a day before I left Iowa, which is not very long.

QUESTION: Any ideas at all as to when?

MR. O'BRIEN: Well, it gives us a hint since we are studying the lower energy particals here, this does give us some hints as to the mechanism, but, I prefer not to go into those here. They are a little bit complex.

QUESTION: As to the mechanism of what?

MR. O'BRIEN: The mechanism which causes this Forbusch decrease which is associated with geomagnetic storms.

QUESTION: What energy cosmic rays are you talking about here?

MR. O'BRIEN: These ones involved here in the satellite are approximately a third of a billion electron volts, one-third of a billion. This is the American billion; 10^9 electron volts.

QUESTION: In other words, about 130 Mev?

MR. O'BRIEN: Yes.

Now, these ones here are of the order of several thousand Mev, the sealevel ones. The balloon observations are somewhere in between the two.

QUESTION: What are the earth-based ones again?

MR. O'BRIEN: We can call it -- do you want it in absolute measurements or relative?

QUESTION: Roundhouse.

MR. O'BRIEN: Ten thousand million electron volts. And the balloon observations are approximately a thousand million electron volts.

QUESTION: Pardon me, you just said the earth ones were ten thousand million - you mean ten thousand electron volts?

MR. O'BRIEN: No.

MR. POMERANTZ: Your earth ones are one to ten billion volts Mev.

MR. O'BRIEN: Explorer VII; on point 3.

QUESTION: Since these decreases are associated with geomagnetic storms, is it possible that these new findings might throw further light on the nature and mechanism of geomagnetic storms and their effects on radio communications, blackouts and things like that?

MR. O'BRIEN: We are sincerely hopeful of this, yes.

QUESTION: Is this an indication that the source of these low energy cosmic rays is the sun?

MR. O'BRIEN: I hesitate to make anything as definite as that. It depends on how we define a source, it is not necessary that the sun actually spits these out and that they travel as particles all the way. They might be just sitting out there in the upper atmosphere waiting for some effect which is caused by something from the sun.

Now, I would like to point out to you that here we are indebted to the courtesy of Drs. MacDonald and Weber and this is a pre-publication result.

QUESTION: Presumably these low energy ones never reach the earth, is that right?

MR. O'BRIEN: That is right, yes. The atmosphere acts as a sort of shield here and this is why the people at ground stations have been limited for so long because you have got an effective blanket of the atmosphere over the earth and only these ones, these very high energy ones, can get through to the ground level detectors. You go up in a balloon to a hundred thousand feet, you can see a little bit better; you go up in a satellite to several hundred miles and you can get down to this rating.

QUESTION: All of these measurements made with the VII were made below the innermost Van Allen radiation belt?

MR. O'BRIEN: Yes, these are -- I prefer not to say so much below as away from the radiation belts.

QUESTION: What was the latitude or the inclination of this orbit?

MR. O'BRIEN: These are all over North America. We have compared -- we knew that a Forbush decrease was going on by using Canadian ground level measurements and we studied Explorer VII measurements over roughly the same region in space where the trapped radiation was not very large.

QUESTION: And did you find that happening just once to date since the satellite was launched?

MR. O'BRIEN: Yes.

QUESTION: In other words, you just had one geomagnetic storm in the interval.

MR. O'BRIEN: We have only got one that we have found to date. There are several geomagnetic storms which have occurred since the satellite went up.

QUESTION: Then do you mean you haven't gotten around to examining the records yet?

MR. O'BRIEN: Yes.

QUESTION: I see.

MR. O'BRIEN: This is not an immediately obvious method of attack.

MR. ROSEN: Are there any more questions on the radiation experiment?

QUESTION: Have you ascertained any more about the composition of the inner and outer belts and particularly this suspected third lower belt, the proton belt?

MR. O'BRIEN: Well, this is still a matter of considerable discussion among us. One of the phenomona I reported was an enhancement of the radiation in between the two radiation belts. Now, this only lasted one day in our particular sequence of observations. If I might draw that.

We have got inner zone here, measuring intensity as we move out from the earth. We have got the inner zone relatively shallow intensities coming up to the Alpha zone.

Now, in this particular sequence I mentioned we have found a double hump type of thing there which only lasted one day while a geomagnetic storm was on.

Now, this sort of thing has been found in several of the Explorer IV studies we have been making so this is not a -- you can't call this a revolutionary new discovery. This whole problem is still being discussed as to how far down you have got to track here, because you see, all these observations are made with detectors which respond just a little bit differently to the radiation and it is quite conceivable that with some detectors you will get this sort of thing.

QUESTION: What you are discussing now, the point that you first made, the sporadic radiation near the inner edge of the outer belt, is that what you are now referring to?

MR. O'BRIEN: No, this is the second point I discussed.

QUESTION: Could you go to the first point, could you go into that a little bit more? What are the relative sizes of these sporadic bursts and so forth?

MR. O'BRIEN: I have a graph here, I don't think-- you probably won't be able to pick it up. These occurred over North America again, the ones we found. We thought the gain intensity versus time. This is now on a very short scale, with that being one minute, that being two minutes. This type of scale now, not a matter of many minutes as here. We have got the outer zone comes up like this. This is as the satellite moves south so that we have got a plot here in time, we have also got a plot moving towards the equator. The outer zone is at high latitudes and as we come down from it we find peaks of this form; this is only a very rough sketch, but these may be almost twice as large. The size of peak may be as large as the size of the more or less background radiation and the actual widths of these peaks are only of the order of seconds, perhaps ten seconds you could take as a working average, and they are separated by approximately thirty seconds, or something like this.

Now, it could be since this is also a distribution in space as we are moving south, you might interpret this as passing through a sequence of zones. We are inclined to think at this stage -- but this is purely personal interpretation and as yet we have got no convincing evidence of this -- we are inclined to think that these are bursts in time rather than bursts in space, simply because this was a certain Greenwich Mean Time that we happened to count that radiation there

QUESTION: Could these be bursts of protons from the sun?

MR. O'BRIEN: They are unlikely to be protons, but even there I would be -- at this stage we cannot answer that definitely.

QUESTION: These were observed at times of solar disturbance?

MR. O'BRIEN: Yes, this may or may not be fortuitous, we just can't tell. Actually, if you are interested, one of these occurred a few hours before we saw this, this latitude enhancement. In the other case, this one occurred when we saw no low latitude enhancement.

QUESTION: I would like to transfer this over to Dr. Newell, whether this doesn't tend to confirm Malcolm Ross, that these outbursts of protons from the sun might produce hazards to any outer space travel for man?

MR. NEWELL: If I could answer that question on a broader basis than which you put it. All of these things that we are looking into are leading to a broad picture of what's going on out in the region around the earth, and although when the radiation belt was first discovered by Van Allen one more or less felt here was a single radiation zone; then later he determined two zones, and now we find that even that has structure, that if you look at this in different energy regions you find zones of particles of those different energies. We are now getting a very complicated picture of which this is a piece, and the next measurement will be another piece, and so on and so forth. So, when you ask Dr. O'Brien here whether these are protons from the sun you ask him to try to fit this observation into a big picture that he as yet doesn't have. This is why he says that you can't be sure. This has to meet a lot of tests.

Now, if I may come back to your specific question, the more of the energetic particles we find, the more protons, particularly energetic particles we find the more of a hazard we find for explorers who are going out into space, so that if this does tie in with Mal Ross' observations and Winkler's observations, my answer is, Yes. My view, if this does tie in. We have got to tie it in.

MR. ROSEN: May we cut this off and go to the next speaker?

MR. LAGOW: Next I would like to hear from Dr. Vernon Suomi from the University of Wisconsin on the radiation balance experiments.

DR. SUOMI: Meteorological experiments on board the Explorer VII measures the thermal radiation budgets of the earth below. Radiation in this case is merely the light and heat most of us are familiar with. The budget is determined by the amount of sub-light which strikes the top of the atmosphere, the fraction that is reflected by the earth's surface clouds and atmosphere and never enters the thermodynamic system and the radiation lost by the atmosphere by virtue of the absolute temperature.

Bodies above absolute zero radiate heat. The net radiation received by the earth's surface depends on the latitude, time of the year, time of the day. These are more or less fixed. The net radiation also depends on the structure of the atmosphere below, particularly cloudiness. The weather affects radiation and at the same time the unequalled distributionof this radiation is basically the source of the world's weather. It is possible to measure or estimate the net radiation which exists on the average and radiologists have done this some years ago.

It is more difficult to do this for a short period of time such as a week or month. Here is where the satellite is of questionable help. Actually the detectors on board Explorer VII are simple, indeed. The hardware necessary to get the information back to the earth is the part that is complicated. It is not so much what they are as where they are that is important.

Explorer VII is making about 4,000 radiation observations here today. Of these about a tenth to a quarter are actually received by the telemetering stations. We must analyze much more data before any statement can be made about the heat budgets of the earth obtained from the Explorer VII measurements and any effect it might have on the weather.

However, the measurements already show details of interest to the meteorologists and perhaps also to the average citizen who is paying for this experiment.

While the satellite was not designed to look at details in the weather below it does indicate clouds or storm areas about a thousand miles across. This shows up readily on the sunlight portion of the earth because of the large amount of reflected sunlight.

However, it is also possible to relate the changes in long-wave heat radiation on the dark side of the earth to positions where cold or warm air exists.

If this comparatively crude experiment can do this, more sophisticated satellites now being planned and under construction can recognize storm systems even on the dark side of the earth.

We have also noted that the variation in radiated heat loss over an area about the size of the United States is about as large as the average variation from pole to pole. What I am trying to say is that just over small areas one gets large changes in the heat loss from the earth.

In addition, mass balloon ascents sponsored by the U. S. Weather Bureau carrying radiation measuring instruments have allowed us to make comparison with the satellite mission.

The engineering portion of the experiment is over and many individuals who contributed to the over-all experiment deserve our heartfelt thanks. Now we are getting a chance to look at the forest as well as the trees as far as the radiation budget is concerned and we are looking forward to this interesting phase of the experiments.

MR. ROSEN: Are there any questions?

QUESTION: Could you repeat the statement which I did not quite follow about over the United States, areas of the United States you were getting heat loss equal from the poles?

DR. SUOMI: What I am trying to say is that if one were to make a plot of the change in heat loss from the earth from pole to equator we lose more heat over the equator than the poles. The difference is about 25 per cent.

But even over a small area of the United States the heat loss from the earth goes through an undulation about the same size as the average change from pole to equator not as large as the whole change but a large fraction of it.

QUESTION: You say we lose more heat from the equator?

DR. SUOMI: Yes.

QUESTION: This is not very astounding, is it?

Would you expect this?

DR. SUOMI: Yes, we would expect this, but what we are after in this experiment is to see if we can relate this to weather effects which might occur.

For example, last November up in the Mid-West was very cold; however, December was rather mild. Now, this weather difference must ultimately be related to the distribution of the temperature, this in turn must be related to a heat as distributed around the earth.

Some of this is caused by the input to the earth from the sun, to be sure; the other is by its loss and this is also redistributed by the weather systems themselves.

So one can make an average picture for a long time; they have to balance out, but if things do not balance out on a short time basis this will give us a key, I think, to the processes.

QUESTION: You have a number of these readings apparently and you also have weather bureau records for this period of time.

Is there any preliminary tie-in between the two?

DR. SUOMI: Oh, yes. If I look at a weather map and look at the satellite record it is possible to relate the variations in the satellite record to the weather map; but you can see that at this stage I am not very confident because the key to it is to go in the reverse direction; to take the variations measured by the satellite and say there are things below.

At the present time we just are finding these relationships, we really need to have much more data and increase the confidence in them.

QUESTION: Going back to this, In November, did you record a high loss of radiation of heat in the earth's surface in the Midwest area?

DR. SUOMI: We had a situation where cold air was

In the Northeast, with a low in the vicinity of Wisconsin, and warmer air to the southwest. As the satellite passed in the northeast, passed from southwest to northeast, the change in radiation, this was all at night, the change in heat loss from the earth went through a violent change as it passed over this boundary.

This was also in agreement with the radiometer sounds released by the weather bureau. So, there is this relationship between air temperatures and types of air and the records received by the satellite.

MR. ROSEN: Does that about do it?

QUESTION: I think you indicated that even as regards the non-sunlight side of the earth, the satellite has made some findings with regards to the heat budgets.

DR. SUOMI: What I am trying to say is that this experiment really was designed to look at the earth in large area. Actually upstairs we do not have a camera, so to speak, we just barely have film, but even this crude device is able to detect variations in the radiation pattern from the earth associated with weather.

This means, then, that as our more sophisticated satellites are placed in orbit it will be possible to obtain cloud patterns and so on on the dark side of the earth as well as on the sunlight side.

QUESTION: Could you establish anything further from that picture that we were exposed to at one time from Explorer VII? Did that show up to be rather significant or was it a --

MR. ROSEN: I am sorry; did it tie in at all with --

DR. SUOMI: I have not attempted to tie this in at all with any of our measurements, Idon't even know if it occurred at the same time.

MR. ROSEN: They certainly over a different area, too, Peter.

QUESTION: That was over the Pacific, yes.

MR ROSEN: Are there any more questions?

MR. LAGOW: Next we will have Mr. Gerhard Heller, from the Army Ballistic Missile Agency, tell us about the temperature measurements and the heat balance on the Explorer VII.

MR. HELLER: The temperature problems of Explorer VII are two-fold, firstly, the design of the satellite has to assure proper functioning of all instrumentation within a specified temperature range of zero to sixty degrees centigrade and secondly, the second aspect of the termal problems is that six temperature measurements were placed as part of the scientific experiments. I am going to talk first about the thermal design problems.

A new concept has been used in the thermal design of Explorer VII. The instrument package is insulated from the rest of the satellite, however; the two conical halves of the satellite shell are allowed to exchange heat by radiation on the inside.

The basic principle of thermal control used is a passive system. That means that it operates without any moving parts. The central instrument column is covered with gold foil and the internal middle parts are highly polished and minimized with heat radiation transfer.

On the other hand, the fiber glass shells are painted on the inside with a titanium paint with a high infrared passivity.

This concept has allowed to minimize temperature extremes of senders located in the skin of the solar cell packages and of the transmitter inside the instrument column.

Temperature measurements evaluated from the records of the ABMA tracking station have shown during the 78 days of operation a minimum of the transmitter temperature of 16 degrees centigrade and a maximum of 41 degrees centigrade.

The maximum occurred during the period of 100 per cent sunlight from the fifth to the tenth day after launching.

It is reasonable to expect that the temperature will stay within the design limits throughout the active life time of the satellite that is hopefully one year.

Variations are mainly due to the time of sunlight during each revolution and due to the changes of the position of the sun with respect to the attitude of the satellite axis in space. The firing time of Explorer VII has been selected such as to minimize the angular deviations of the sun from the satellite equator.

I might add it actually will not stay constant but will, the position of the sun will oscillate in sinussoidal fashion around the mean position at the equator of the satellite.

In the time span from first of November to 15th of December the temperatures of the transmitter stayed almost constant at 18 degrees centigrade with variations of not more than plus or minus two degrees.

Presently Explorer VII goes through another period of 100 per cent sunlight from the 25th of December to the first of January.

The temperature requirements for the instrument package are determined for the upper limit by the transmitter and the batteries and for the lower limit by the batteries that would freeze out if the temperature drops below zero degrees centigrade.

The solar cells would allow higher temperatures than 60 degrees, if not, their power output will decrease with increasing temperatures.

In addition to the effects of the sunlight-shadow periods and the attitude of the axis to the sun, other important factors on the thermal problems of satellites are the variations of the radiation environment from sun, albino and earth radiation and the change of surface characteristics to the exposure through the environment of space.

Now, to the second part: The experiments of Explorer VII include the measurement of fixed temperatures by senders whose output is transmitted through the telemeter on 20 megacycles. Besides the transmitter temperature or diminution the following temperatures are monitored in a continuous sequence, skin, solar cells, batteries, Geiger Mueller tube and a hemispherical sender of polished gold on the satellite equator.

All experiments are working well and a tremendous amount of valuable information is obtained from the telemetered records of the tracking stations.

Evaluation has been started on the temperature data of the ABMA tracking stations. Correlation with data from other stations and with results of the University of Wisconsin radiation experiment is in progress.

Results from the analysis of the thermal experiment will increase our knowledge of the environment in space and its effect on satellites and will in turn allow us to improve the thermal design of future satellites.

Up to now Explorer VII has given the most complete data coverage of temperature measurements, the experiment is considered very successful.

MR. ROSEN: Peter?

QUESTION: Based on what we have received so far, in the temperature measurements, what improvements would you possible suggest for a future satellite such as this? I say it may lead to improvement, obviously it will. I am wondering if you found any failures or anything that could be improved so far?

MR. HELLER: Well, actually this thermal control worked quite satisfactorily, but requirements for a future satellite as such will increase. We will very likely have to now design down the temperature limits and have more difficult requirements fulfilled. So this information will help us do that.

QUESTION: You said something about you hoped that the active life of the satellite would be one year. You meant one year for giving out information? As I under= stand it, the lifetime of this aloft is about twenty years, isn't it?

MR. HELLER: The lifetime is much longer.

MR. ROSEN: There is a timer in it to make it stop transmitting in about a year.

QUESTION: Could you draw up a little sketch of this inner and outer shell?

MR. HELLER: I don't know whether everyone can see it. The satellite, what I call equator is this short cylindrical part in the middle; well, it is rotating around this axis, so this corresponds to the equator; this is the upper shell and this is the lower shell; and the main purpose of assuring this radioactive transfer from one side to the other is to minimize temperature extremes. The sun during the oscillation it makes around this equatorial mean position is at the position now, right now it's somewhere here and almost no light would fall on this, and this shell would run considerably hotter then the lower shell, and again this would affect all temperature extremes in the satellite.

This rate of transfer is considerable. Actually, it's quite interesting to learn during the process of this design that it could not be conducted from one side to the other even with very heavy copper bars. The radiation transfer is quite effective.

MR. ROSEN: Yes, Al.

QUESTION: You mentioned a little closer control might be necessary to future satellites. This appears to me, a 25-degree variation to be very small, even much smaller than you will find in a few thousand feet on earth.

MR. ROSEN: Did you mention that?

MR. HELLER: That is right, at present this is the transmitter temperature, the inside, which is now in a favorable position. Now, other temperatures definitely are higher and also lower at different parts of the satellite.

QUESTION: Do you have figures for the high and low skin temperatures?

MR. HELLER: Yes, we have some figures. I hesitate to quote these for this reason, so far we have not a complete cycle which we try to get from all stations so we just -- the temperatures of the skin go to quite some extremes, go up and low, to even temperatures below freezing, so any point we have measured does not give a complete picture because it's just an arbitrary point of such a curve. What we try to do is to run a computing program where we fed in information, correlated this, and then we acquired certain confidence that a specific measurement makes sense.

QUESTION: Could we have those even if they are not complete?

MR. HELLER: Well, they are presently not available in a form. You certainly could have them.

MR. LA GOW: They could be made available?

MR. HELLER: They could be made available.

QUESTION: Could you give us a rough estimate, your memory of it?

MR. HELLER: We have temperature measurements on the skin -- maybe I can sketch it somewhat.

A typical cycle of a skin temperature during one revolution would look somewhat like this, this being the shadow area and we enter sunlight, we have a steep rise and the maximum is not at the end point, at the end of the shadow again because the maximum is where all radiation total is at a peak including the albedo and we have a drop of the albedo in this region. And then, of course, a steeper drop up in the shadow.

Now, this picture is again dependent on the attitude of the satellite with respect to the sun; so, actually if we would plot the curve for the next day, it might be somewhat different. Also, we have additional changes that this shadow period changes, so in 100 percent sunlight all temperatures are higher and it almost goes through a cycle like this. What we have obtained are, of course, points here, here, here, and so on.

Now, I'll quote a figure which I said cannot give any detailed figure, cannot get a complete picture. We have a value like this and we have values as low as 5 degrees centigrade, this 53. But again we do not know exactly. I put these arbitrarily on the curve here. We do not know exactly where they are and how they correlate, and so if you obtain information it will be very sketchy and will be hard to get together what it actually means. What we are trying to do is to establish such theoretical curves and get, then, measurements that would follow this specific computed curve. We expect some deviations from these to the weather variations and others, and hopefully we can also explain these by correlation with the radiation equilibrium. But unless we have done this, it is a very sketchy picture.

MR. NEWELL: I might make a general comment in here that when the satellite business began, one of the main problems was the question of whether one would be able to control temperatures, and the serious question was whether one would be able to control temperatures in the interior of the satellite in which batteries, transistors and other equipment would operate properly. The results, success of this satellite, and on others, indicate two things: one, that we have been able to handle the problems that we faced so far; and two, looking forward to the future one may expect to be able to produce temperatures in limited regions, in cavities, for example, in Deer Flasks that stay constant for a fraction of a degree for such things as velocities and so on.

MR. ROSEN: All right.

MR. LA GOW: Next we will have Dr. Martin Pomerantz describe the heavy cosmic ray experiment that he and Dr. Philip Schwed have done.

MR. POMERANTZ: I am simply acting as spokesman today for a group consisting of the group of Philip Schwed, who is in the audience and the late Dr. Gerhart Groetzinger, who passed away before this satellite was launched.

This experiment was designed to investigate the heavy primary cosmic rays. These consist of heavy atoms stripped ofexternal electrons and endowed with very high energies.

They come from the far reaches of our galaxy and have traveled vast distances through interstellar space before reaching us.

We can learn much of fundamental interest by studying their characteristics. For example, the chemical composition of the sources of cosmic radiations reflected in their abundances.

It is remarkable that heavy primary cosmic rays can withstand the process whereby they acquire their energies without splitting up any theory of the acceleration mechanism must account for the characteristics of this component.

Furthermore, the fact that they have survived their long journey yields information about conditions in cosmic space. The particular fraction which we have set out to study comprises those elements heavier than boron on the periodic table.

We wished to determine the energy distribution of these particles, that is the population in terms of the energy, and the possible changes with time.

For the former purpose it is necessary to know with considerable accuracy the variation of their rate of arrival with geographic location.

record

The ~~data~~/ is accomplished obviously by monitoring the rate of arrival at fixed locations over extended periods of time such as is feasible with this particular satellite.

The detector employed is a so-called pulsed ionization chamber, and its use enables us to select by means of appropriate electronic cervix cosmic rays, the heavy primary cosmic rays, even in the presenceof a much larger background of radiation of other types. This is the first occasion on which this sort of detector has been used in a satellite experiment. It has proved especially well adapted to this application because it combines a high sensitivity and a great capability for discriminating against interfering effects with an extremely low weight.

The associated electronics, which is less than a pound in weight and is actually a fraction of the size of the detector itself, performs the function of laboratory equipment fifty to 100 times as heavy. The results to date have shown that the desired performance was indeed attained.

Specifically we have been able to obtain a preliminary representation of the energy dependence. As mentioned before, this has been accomplished by determining how the rate of arrival depends upon geographic location of the satellite and, for example, at high latitudes the counting rate is approximately ten times as large as it is at the equator.

It may be remarked that the capability of making such a large scale survey at a rapid rate is one of the great advantages of a satellite vehicle for investigationsof this type.

This device permits a very critical examination of the effects of the earth's magnetic field one charged particles approaching the area and this is very important for a number of very practical reasons, especially in the communications field.

Fluctuations in intensity probably associated with stormsin the sun have been observed but have not yet been studied in any detail.

We expect that the final analysis of these fluctuations will exist in our understanding of the effects of solar influences on cosmic radiation.

In particular, we shall be especially interested in seeking to detect any heavy nuclei emitted directly by the sun -- an occurrence known to transpire in the case of hydrogen, the most abundant component of cosmic radiation obtained when a very intense part of the trapped radiation belts are considerably higher than normal.

The increase is much smaller than that registered by other types of detectors used in the studies of the belts. The existence of this residual counting rate appears to be of considerable significance for understanding the nature of the particle in the belt; and we have initiated further investigations to explore the possibility.

As of the present moment we have not had enough data and the calibrations have not been performed which are required to study the subsidiary question.

MR. ROSEN: Is your instrument calibrated to give energies?

MR. POMERANTZ: It is calibrated to detect particles which have an atomic number larger than a certain minimum value. Carbon is the lightest, z equals 6, atomic No.6, and the particles which it detects are relativistic particles, that is the particles moving with the velocity of light.

It would reject the lower energy particles.

QUESTION: Aren't your most powerful cosmic rays the heavy nuclei?

MR. POMERANTZ: Well, if we talk about the usual procedure to cause the energy per nuclei, per particle in the nucleus, these heavy nuclei have energies per nucleus comparable with the nucleus.

This mean, for example, for an atom like iron, that the highest energy one would be able to carry, what is it, 56 times the energy -- whatever the atomic weight is, as compared with the proton.

QUESTION: Howmany elements haveyou detected and what are they?

MR. POMERANTZ: Well, there are groups of carbon, nitrogen and oxygen, and heavier. In other words, there are three regions, minimum atomic numbers, so that one can then determine the whole spectrum within that limitation. The lowest one is 6, 9, and 16, roughly.

One cannot get a very sharp cut off in this calibration because of variation in path length through the chain and things of that sort, but these are the ranges.

QUESTION: Have you detected iron?

MR. POMERANTZ: One cannot specifically say whether there is a nucleus of iron, but there have been particles with atomic numbers larger than 16, some of which certainly could have been iron.

There is no way to identify them.

QUESTION: Well, carbon, nitrogen and oxygen are the only ones you can definitely say you have detected, is that right?

MR. POMERANTZ: That is the lower limit, carbon is the lower limit. In other words, the lowest channel would detect everything heavier than carbon, the next lowest channel would start at a higher place, so they are not mutually inconsistent.

In other words, if a particle is iron, it would send off all three channels.

QUESTION: How would you relate the study in which you were involved, the experiment in which you were involved as it relates to communications with that of Mr. O'Brien?

MR. POMERANTZ: Well, one of the uses, if I may use that term, of cosmic rays is that they serve as a probe having passed through the regions surrounding the earth, so they go through all sorts of magnetic clouds, magnetized clouds, things of that sort, which on occasions can inhibit cosmic rays, prevent them from reaching the earth.

In this sense it is of great interest to compare the changes of the type that Mr. O'Brien has discussed with those on the heavy nuclei which represent the much different energy range of particles.

So, in this sense the investigations are quite related. One point which I should emphasize is that we know that the earth's magnetic field, we understand how the earth's magnetic field affects the orbits of incoming particles and recently we have begun to understand that the ideal conditions that would apply at the earth's field could be represented by a bar magnet, by a dipole breakdown, and it is of great interest to investigate this in detail, the departures from this ideal condition.

Now, these particular heavy nuclei, since the detector is not so susceptible to the variations of altitudes thatother detectors are, afford one an excellent method of studying, of plotting essentially the places where a given magnetic field exists.

In other words, isomagnetic lines, essentially.

QUESTION: There has been some question about the possibility of primary cosmic radiation being detrimental to life on the moon.

Is your experiment likely to shed any light on this?

MR. POMERANTZ: Well, I am afraid this is an area beyond our competence, all our experiment can do is to indicate how many of these particles there are and what their energies are, and then it becomes a biological question to decide whether there are such effects.

I will say this, that the characteristic of heavy nuclei as compared with the protons is that they would ionize very much more densely in a limited region than a proton would since the ionization goes as the atomic number to the second square, z^2, so in terms of ionization they are very intensely ionizing.

What the effect of this is I would hate to guess.

QUESTION: Have you any estimates on how many and what energies they are?

MR. POMERANTZ: The heavy nuclei comprise something like the one per cent of the total cosmic ray intensity that is the galactic intensity, what you would find outside of the radiation belts.

That number is roughly one per square centimeter per second, so this would be one per cent of that, this is a ball park figure, it depends on where you are with respect to the earth's magnetic field, so they are not very plentiful in terms of the intensities that you would find, say, in the radiation belts, for this reason one is hopeful that they may not do very much.

QUESTION: This estimate of one per cent I think is one that was made before this was sent up.

MR. POMERANTZ: Yes, this one per cent is -- this order of magnitude is certainly well-established and this is certainly not a new result.

QUESTION: You have not found anything to disturb that estimate?

MR. POMERANTZ: No; I would say that the kind of counting rate is consistent with the expectation on that ground.

QUESTION: Does that hold true for energies also?

MR. POMERANTZ: In terms of energy, we find that what we measure is essentially the population density in terms of energy and we find that the energy spectrum goes roughly something like one over the energy; this is very crude.

Inother words, there would be ten times as many particles above a billion volts, if you take all of the particles, as above ten billion volts, that kind of order of magnitude. That is very crude.

MR. O'BRIEN: Might I add something here which may or may not clarify things?

One of the particular advantages of Dr. Schwed's experiment as I see it is that previous observations of these heavy primaries have been made with balloons.

You do not get heavy primaries at sea level; they fade out so you have to go up in balloons or rockets. Balloons will only stay up there, they are being very favorable to you for 24hours or so. Whereas this present experiment is just going to go whirling around collecting these heavy primaries for about a year and this is quite a fundamentalimprovement.

MR. POMERANTZ: Thank you.

MR. LA GOW: If there are no more questions on that, we will go to the solar radiation experiment. Unfortunately, Dr. Friedman and Dr. Chubb could not be here today, from the Naval Research Laboratory, that sponsored this experiment, so Dr. Chubb prepared a statement which I will read to you.

The solar radiation experiment in Explorer VII has been responding almost exclusively to trapped particle radiation rather than to X-ray and Lyman-alpha radiation from the sun. One can conclude from the experimental data that an experiment to monitor solar X-ray radiation must either be carried out below an altitude of 300 miles, or that means must be provided to protect the thin window radiation detectors from incident electrons. Despite the problems associated with trapped electrons, it is felt that valid solar data may yet be obtained when records from the Pacific are studied.

QUESTION: In other words, After all of our futile attempt to put up that experiment, it didn't work?

MR. LA GOW: Well, this is not completely true yet because only a portion of the records, a fairly small portion of the records have been examined and I would like to ask Brian O'Brien to comment on this point.

MR. O'BRIEN: As you may have gathered, our main point in putting our apparatus up is to study the trapped radiation. Now, we are getting quite a lot of measurements of trapped radiation. Unfortunately, for every measurement we get, this means that the solar detectors don't get a measurement because this trapped radiation is interfering with it. So they just have to analyze their data and actually sort through so as to select periods in which there is no trapped radiation or negligible trapped radiation hitting their detectors. So although this is a nasty thing in that it reduces the total percentage of time that we are effective, although I use that word "effective" in a little bit of a dubious way, I think they are always effective, although the trapped radiation does reduce the time of measurements, nevertheless they still have times of measurements, just a smaller amount.

MR. NEWELL: To pick this point up further, you raised the question by saying, "It was put up and it is not successful." You recognize that the orbit of this satellite is eccentric and there are portions in which the equipment is below the radiation belt enough to get the data they were after originally.

They just haven't worked out this part yet. When they do they expect they will have what they were first after. But now in addition they are getting information on the radiation belt, in particular, since the ionization chamber continuously picks up these radiation belt particles when it is in the belt, it will give you quite a bit of information on the structure of the inner part of the radiation belt. So if you want to look at it another way, this is being more successful than we had expected.

QUESTION: I didn't mean to be critical. I just meant it was one of the first things we were going to put up.

MR. NEWELL: It had a long history of putting it up.

QUESTION: What do you mean by getting information of the structure?

MR. NEWELL: You see, if it is recording the radiation belt particles, as long as it is in the radiation belt and drops down to a different counting rate as the satellite comes out of the radiation belt, this then, you see, will give you the location of the lower edge of the belt. If you have this over a space of time, you have some interesting information.

MR. THOMPSON: This also gives you some look at some low-energy radiations which are not picked up by the van Allen counters.

MR. O'BRIEN: I might add that we can't lose from those, because Dr. Pomerantz and Dr. Schwed are measuring this for us with their detectors and some of the Naval Research Development people. So we are quite happy even though a little bit apologetic that things are confusing; they are issues.

MR. ROSEN: We have got to move a little bit faster.

MR. LA GOW: I will report on the status of the micrometeorite and the erosion experiment. This experiment is conducted to evaluate some of the hazards in the space environment . It consists of three evaporated cadmium sulphide conductors which are covered with thin but optically opaque films. The erosion of these surfaces by either high velocity molecules or impacts from micrometeorites would produce openings in the covers. The admitted sunlight would change the electrical resistance in the cell in proportion to the area of the hole.

Analysis of the telemetered records to date are incomplete and an examination of selected records from the first months show the following:

1, that approximately one-half of 1 percent of the area of one cell was admitting sunlight. This puncture occurred during the launch phase and hence is not expected to be from a micrometeorite. No further penetrations or erosions have been noted to date. The telemetry equipment in the cell, and the temperature sensor, to measure the temperature of one of the cells, has functioned properly.

This experiment was telemetered on the 108 Mc tracking transmitter which was last tracked on December the 4th this year when its chemical batteries were exhausted.

QUESTION: I wonder, would you clarify that one-half of 1 percent. Did you say one-half of 1 percentof the --

MR. LA GOW: Of the total area of one of the cells.

QUESTION: What size would that be?

MR. LA GOW: The cell is 3 millimeters in diameter.

QUESTION: What do you suppose that --

MR. LA GOW: Very small.

QUESTION: What do you suppose that puncture could have come from?

MR. LA GOW: I don't know, yet.

QUESTION: Well, I know you don't know, but is there any wild idea or theory on this launch phase?

MR. LA GOW: The reason I say I think it's associated with the launch phase is because it happened so quickly after launch and there not being subsequent punctures. It could have been a dust particle from the vehicle, possible.

MR. ROSEN: We move on to the next one.

MR. LA GOW: Next, I would like to call on Mr. Arthur Thompson from ABMA to report on the uncovered solar cell operation.

MR. THOMPSON: I would like to point out that I am not personally the sponsor of this. It is really a group sponsorship, ABMA and some of the people from Fort Monmouth Signal Corps Agency. The objective of this experiment was to determine the effect of space environment on unprotected solar cells. The results so far on the objective of this has indicated that the cells are operating properly after ten weeks of operation in the unprotected space above the earth. However, you have got to point out here that the apogee of this satellite is about 673 miles, this is below the, say, the lower radiation belts as they are described generally. Of course, we have lower energy ones as we note from the NRL experiment down to where we are, but the cells themselves are not being exposed to the high energy particles that are trapped in the radiation belts, so what we are doing is actually proving that the micrometeorites, after ten weeks, haven't seriously affected the cell.

It will take an experiment on a satellite which goes to a much higher altitude to determine the effect of radiation.

That is all I would have on that.

MR. ROSEN: Bill?

QUESTION: You mean that the quartz crystal window that we have used or are using for the protected cells also offers protection against radiation as well as against micrometeorites.

MR. THOMPSON: Yes.

QUESTION: By about what factor? I mean how much does it reduce the energy level?

MR. THOMPSON: Brian, could you or Dr. Newell handle this one?

MR. O'BRIEN: I think the point we have to consider here is that damage can arise through discoloration which will absorb out the light which wants to get into the cell to generate the electricity. You can also change the surface if you bombard it with particles. This is being done in the laboratory.

MR. ROSEN: This is the carbon-base plastic?

MR. O'BRIEN: Quartz. In an unprotected one, if you expose it to intense radiation, then the surface radiation damage, where radiation is possible, there is possible radiation damage.

QUESTION: Haven't the Soviets also sent up some with unprotected cells and haven't they found them to be operating, or operating quite well?

MR. THOMPSON: I will take that one. Yes, they had one on Sputnik III which lasted something like two months -- let's put it this way, the report that we got says that it was still going properly something like two months; however, they have the same problem that we have, their apogee was something in the order of 1,200 miles but it didn't stay -- of course, that is in the radiation belt, but it didn't stay there very much of its lifetime, so three months of operation or so still doesn't indicate very many hours in the radiation belt.

MR. ROSEN: The perigee of Sputnik III was 135 miles and the apogee was 1,167 miles.

QUESTION: So, for the time being I presume we will go on protecting the cells we are relying upon for power until we get some additional data that will indicate we can save that weight.

MR. THOMPSON: Satellites which have to penetrate the radiation belts.

QUESTION: The apogee of this, most of the life has been at what altitude, around 700?

MR. THOMPSON: You are speaking of our satellite?

QUESTION: Yes.

MR. ROSEN: 673.

MR. THOMPSON: Is the apogee.

MR. ROSEN: John?

QUESTION: Let me ask a confused question here. What is the distance of the inner and outer radiation belts and I don't see that you are going through them. How are you making measurements here?

MR. O'BRIEN: I think I had better draw a picture here.

This is going to be an extremely rough picture. We have the earth; we have the North and South Poles, as so; we have essentially what we call an inner radiation belt somewhere in there, in general about the equator, at varying altitudes at about a thousand kilometers average, something of this order. We have the outer radiation belt which comes around like so in the form of horns. The distance here in miles varies between, say, 300 and 700 miles, depending on which side of the earth you are on.

QUESTION: Distance between what?

MR. O'BRIEN: Distance from there to there.

QUESTION: That is somewhat lower than the earlier figures of about 2,000 miles, I believe, wasn't it?

MR. O'BRIEN: Well, you see this nasty point here, this depends upon the magnetic field and your magnetic field is displaced from the center of the earth, the center is further out on the one side of the earth than it is on the other side.

In this satellite at a thousand kilometers -- call it 600 miles -- we are picking up quite strong inner zone over Johannesburg and over San Diego.

QUESTION: At 600 miles, you say?

MR. O'BRIEN: Yes. This is right up near the limit, though. This is up near the apogee. This is our outer zone which more or less filters down in gradually decreasing intensity. Of course, what I have drawn here are the regions of maximum intensities and you have fringe ratings going on, you gradually come down. This is why earlier I drew curves of intensity versus position which went like that rather than like that and like that.

Okay, we have got these fringe regions. Now the satellite at present is somehow or other cutting like so. This is very, very rough, but the important point here as far as the unprotected solar cell is concerned is that it spends quite a deal of time in these regions and a little bit up here outside the most intense radiation zones.

QUESTION: What is the distance out from the earth's surface of the outer?

MR. O'BRIEN: This one, 28,000 kilometers; call it 15,000 miles.

QUESTION: Fifteen?

MR. O'BRIEN: 15,000 miles, yes.

QUESTION: And then it is --

MR. O'BRIEN: It stretches right down here to altitudes of only a couple hundred miles.

QUESTION: Your satellite is cutting through the horns.

QUESTION: What is the center of the outer belt from the earth's surface?

MR. O'BRIEN: Sorry. The figure I gave was actually from the center of the earth. Do you mind the center of the earth?

QUESTION: Or surface.

MR. O'BRIEN: The surface of the earth, it is around 12,000 miles.

QUESTION: But the horm comes down to about 200?

MR. O'BRIEN: The horn comes in here, yes, into there. We often talk about this as being the equatorial region because the earth's magnetic field curves in the same way.

QUESTION: These are the magnetic poles you have shown here. When did we discover that the center of the earth's magnetic field does not go through the center of the earth?

MR. O'BRIEN: The magneticians on the surface of the earth discovered this quite a long time ago. They have been trying to fit their measurements to a given form in here. Some cosmic ray observations have reinforced this and the fact that you get this inner zone at different points is a further evidence. It is by no means the only evidence.

MR. THOMPSON: The Argus experiment demonstrates it.

MR. O'BRIEN: The Argus experiment was another demonstration of this because the Argus went further out on the one side than the other.

QUESTION: You say it is 12,000 miles. Then how far out does the second belt extend?

MR. O'BRIEN: This is the one in which the actual displacement of this magnetic bar, if you like, is a couple of hundred miles, you see, and then this couple of hundred miles is added to --

MR. ROSEN: That is 650.

MR. O'BRIEN: The order of magnitude 300 to 500 miles or upwards of that, actually.

MR. NEWELL: Wasn't the question how far out --

QUESTION: I am trying to find out how thick the outer belt is.

MR. O'BRIEN: Well, this varies with solar activity. I am sorry. I thought you were talking about this. Two space rockets have shown different thicknesses here. One came right out here somewhere and the other one came in, and the Explorer VI observations actually sampled out here, too, and they are giving very useful information about this.

QUESTION: How far are they? What is the thickness? Does it go out to 35,000 or 50,000?

MR. POMERANTZ: They went to something like --

QUESTION: Mr. Van Allen says it goes as far as 50,000, but with fluctuations depending on solar activity.

MR. NEWELL: That is right.

MR. O'BRIEN: I think one can't lay down rigid limits on this because you have to define what you mean by the belt at first, and I said there are very extensive fringe regions.

QUESTION: How would you describe the path of this satellite with regard to the two radiation belts in general?

MR. O'BRIEN: Very, very roughly, that is it there.

QUESTION: What I mean is would you consider that as putting the satellite through the major portion of the inner belt and at least the fringe portions of the outer belt; how would you describe it?

MR. O'BRIEN: I don't think it stretches really into the heart of the inner zone, the inner belt here. But we are still a little bit uncertain about this. We don't quite know where that is either.

MR. NEWELL: I wonder if I could drop in a few illustrative words here. The problem of the radiation belt is rather confused because of the fact that you have a wide range of particles, a variation with time. By way of analogy, if you were to look at the sun's visible light through a red piece of glass, things would look red, you just see red. If you look at it with a green piece of glass, you see the green; and yellow, yellow; and so on.

In the same way here, if you look at the radiation belt with equipment that can detect nothing below a hundred Mev, for example, particles, then you will see one structure for the radiation belt. If you look at it with other equipment that can see in a different range, then the radiation belt will look different again. And if you look at it with, let's say, equipment that can go down into the kiloelectron volt range or lower, you will get an even more difused picture of the radiation belt. But with that qualification I would think you could say we have seen that the radiation belt and all its complexity extends out to maybe 50,000 kilometers. Wouldn't you say that is right?

MR. O'BRIEN: Yes.

MR. ROSEN: Any more questions?

Fine. This is our last presentation.

MR. LA GOW: We have one more presentation here. Mr. Josef Boehm of the Army Ballistic Missile Agency will summarize for us what the mechanical problems encountered in devising this satellite were.

MR. BOEHM: Explorer VII was engineered to serve as an orbital carrier for seven scientific experiments which we have just studied.

It was a very challenging engineering task to incorporate the great number of requirements into one mechanical and electrical system. The complexity of the system, the applied technology with respect to a microminiaturization and the light weight design make this carrier the most efficient and sophisticated satellite of longest lifetime which was ever placed into orbit by the United States so far. But despite the apparent complexity, great effort went into establishing a relatively simple and clear layout which now guarantees exact and unbiased measurements.

I shall point out a few of the essential development phases.

The design development of the satellite was greatly influenced by a series of requirements which resulted partly from the specific mission and partly from the applied booster system which we had to use and which we used. The most conspicuous requirement given by the mission was that the tumbling of the satellite had to be avoided in any case.

Fortunately we can say that telemeter data so far indicates that the satellite is fine, keeping its attitude, and there is no trace of tumble noticeable. The fact to shape the satellite in such a way that the satellite maintains the attitude of the spin axis was the greatest influence to the shape the satellite finallyobtained.

We needed a disk-shaped mass distribution, a disk-like mass distribution of suspended stabilized body.

The shape of the satellite was in addition determined by providing suitable attachment possibilities for the elements of the different experiments and to have sufficient area for the solar cell areas.

Quickly I would like to go over the configurations. You have those which Mr. Heller showed with his photography. The satellite itself consists of a truncated double cone, joined by a cylindrical center ring.

It is thirty inches long and 30 inches wide at its equator. The instrumentation is assembled in modules of six inches in diameter and of varying thickness.

The assembled modules have a column located around the spin axis. Iwould like later to make a sketch of it.

The desighers of the satellite had further to consider traces which occurred through the thrusts and the centrifugal forces. We have spin. The thrust as you know of the configuration has 450 rpm spin. We had to consider a great variety of vibrations. We had to see that the eddy currents were minimized as much as possible because the body is spinning in the earth's magnetic field.

The special problem was the selection of the material for vacuum condition. We had to introduce as a result of design quite a number of changes going into quite a number of different kinds of material.

And the last point I would like to mention, which was explained by Mr. Heller, the influence of the temperature control to the design. There was quite a lot of cooperation required in order to achieve the temperature control.

The total weight of the explorer VII was 92.3 pounds at launching. I have here a breakdown of the components which I shall just quickly read.

The structure of this 92.3 pound satellite weighed 29.6 pounds. The instrument package, 14.4; the battery supply, 15.9; solar cell arrays 18.2; separation decision, 2.8; the detectors and sensors, three pounds; balancing weights, .7 pounds; the antenna system, 108 mc. and the 20 megacycle antenna, 2.2 pounds, the other is 3.2.

Potting and wiring 1.1 pounds, and the surface coating paint .9 pounds. The sum is 92.3 pounds. The altitude requirement for the satellite made also the separation of the empty shell of the last stage motor from the satellite necessary.

Therefore, a separation device was inserted between satellite and the fourth stage. As planned, there was no dynamic disturbance caused at separation. The transistor communications systems employed two transmitters, one solar cell powered operations at 20 mc.

Both communications systems worked perfectly from the beginning. The 20 megacycle system went into operation after its antenna wires were unreeled and orbited to a diametral length of 24 feet. This happened right after separation of the last stage shell.

So far we have measured only a very small spin from the original spin. The spin as the last stage had been fired was reduced again by the change in the mass distribution due to the unreeling of the fullantenna as I said to 24 feet total length.

Now, the most important prerequisite for the success of Explorer VII was to establish an extremely high degree of reliability. This was achieved by subjecting the satellite and its components to a most comprehensive and severe test program which included all functional and environmental conditions or requirements.

Several prototypes of the satellite were prepared for mechanical, electrical and thermal test groups. Space conditions were simulated for the instrumentation by applying the temperature vacuum "soak" tests of long duration.

Just to see how the instrumentation would behave later in orbit, the separation of the last stage shell from the satellite was simulated to check the dynamic behavior during the process of separation.

Separation of the extent of 20 megacycles antenna system was tested in a special vacuum chamber.

After nearly two and a half months of successful operation of Explorer VII it can safely be stated that a remarkable progress in space technology is achieved. This was only possible with the solid teamwork which was performed at the Developments Operations Division of ABMA, the main load of the work being carried on by the Guidance Control Laboratory, and I would like to emphasize and mention here that I am just a representative speaking about the work and contributions many people have given to this project.

QUESTION: What is the spin rate and rpm?

MR. BOEHM: The original spin rate was normally 450 rpm of the cluster. And the spin rate went down to approximately 360 rpm at the beginning of orbiting when the antenna system was unreeled. We have now a small drop, let me say between 5 and 10 rpm, which means that the spin decay is a very slow process and we are positive we have sufficient spin-for-spin stabilization for the planned one year of operation.

QUESTION: What ;was it now, sir. I didn't hear what you said. What is the current spin rate?

MR. BOEHM: I know it was originally roughly 360 rpm; it went down to about 300.

MR. THOMPSON: It is about 350; 348 to 350.

MR. ROSEN: Are there any more questions?

QUESTION: I have one. In summing up I would like Dr. Newell or Mr. LaGow, or someone, to give us an overall summary of what the success of this is.

MR. ROSEN: Dr. Newell, would you reply for the gentleman?

MR. NEWELL: Well, I will attempt a summary. I may turn to Mr. LaGow to fill in some of these things.

The Explorer VII satellite, by way of summary has been successful in its design as you have heard from Mr. Boehm and Mr. Heller. The temperature control is working as it should; the temperatures for the instruments are within the proper ranges. The structure is as it should have been; the spin rates desired, mainly about 350 rpm, were achieved. The equipment seems to be working with the desired reliability, and we can hope that the satellite will operate for the full year that it is supposed to.

As to the scientific measurements, the equipment in this case is also working as it should, and in the case of energetic particles, the SUI equipment is operating properly, and there were three main phenomona observed. I will ask for three brief statements from the SUI man right here as to what those were.

MR. O'BRIEN: One phenomona was quite simply that we observed bursts of sporadic radiation on 20 occasions; another was that we observed a temporary center of increased radiation inbetween the two radiation zones, and this is an extension of Explorer IV work; and the third phenomona is that we have been able to push the study of the Forbush decrease to rather lower energies.

MR. NEWELL: Picking up with this, the energetic particles measurements include observations on the heavy particles in cosmic radiation; that means those that have an atomic number greater than 6. These measurements are divided into three groups, all of those greater than 6, all of those greater than 9, and all of those greater than 16. So that in effect by comparing these two measurements you can get those between 6 and 9, those between 9 and 16, and then those heavier.

Are there any other specific items you want to add?

MR. POMERANTZ: Well, the energy distribution of the heavy nuclei has been determined and the special distribution, the geographical distribution, has given data which is related to the effect of the earth's magnetic field on incoming charged particles, and fluctuations which have not yet been studied in detail. Temperature variations have also been observed.

MR. NEWELL: Thank you.

In the radiation measurements there is the NRL radiation experiment which refers to the solar X-radiations; these counters are operating properly and are obtaining data on the lower edge of the inner radiation belts, and presumably are obtaining the solar X-ray data that they were sent up for over portions of the orbit which have not yet been analyzed.

This is the reason I say presumably in connection with that statement.

In the case of the meteorological experiments, the Suomi radiation balance experiment, this equipment, too, is operating as it should. Something like five or six thousand measurements are being taken per day, of which from 10 to 25 percent are being read off at the recording stations. I will ask Dr. Suomi to add to this the main points obtained from his measurements.

MR. SUOMI: The main points for the meteorologists to analyze this enormous volume of data -- we have for the first time a look of a good aspect of the world's weather on a world-wide basis or almost a world-wide basis and while I did comment about a few details the real story will come with the analysis on a world-wide basis forlonger periods of time.

MR. NEWELL: Then picking up again we have the micrometeorite measurements. the indications from the equipment in the satellite are that micrometeorite puncture and erosion for the period of time that these observations were made is not a great engineering hazard.

This still leaves open the problem of what these effects may be over long periods of times, say for many years, and the plan is to continue such measurements in other NASA satellites.

I will ask Mr. Lagow if he has anything he wants to add.

MR. LAGOW: I think that covers it. Dr. Newell.

MR. NEWELL: Then I would like to make a general comment. You have now been hearing about an important and versatile satellite. You have been hearing from the scientists who are doing the actual experimenting.

It must be clear to you how much of a team enterprise this sort of thing is. Without a joint effort of many people such a satellite experiment or collection of experiments cannot get done.

Now the question naturally arises as to how this teamwork can be broadened. In particular, how can the participation of the scientific community be enlisted on the broader basis.

In this connection NASA would like to make the following announcements: the individual experiments on this team have made available to us the complete telemetry codes for their experiments and instructions on how to use them.

This material would make possible to scientists around the world who are able to record the telemetry data that they can reduce this data for themselves and participate in the experiments. We will proceed to assemble these telemetry codes and instructions and in the near future will be prepared to accept requests from scientists around the world, both in our country and in other countries, for copies of these telemetry codes and the accompanying instructions.

I think that winds it up.

MR. ROSEN: All right, ladies and gentlemen --

QUESTION: When you say in a position to accept requests -- is this an indirect way of saying you will send it out to any qualified scientist who asks for it or will there be exercised certain limitations?

MR. NEWELL: We will send it out to any qualified scientist who requests it.

QUESTION: On either sideof the Iron Curtain?

MR. NEWELL: Yes, sir.

MR. ROSEN: Anyone who can receive it.

QUESTION: Is there any particular area of the world where you are much more interested where you have a big gap where you would like to get scientists than in any other?

MR. NEWELL: I would like to ask the individual scientists to answer this question.

MR.ROSEN: Dr. O'Brien apparently has a few gaps.

MR. O'BRIEN: Well, these examples of gap, -- this is not intended to be a comprehensive list --

MR. ROSEN: Or an invitation?

MR. O'BRIEN: Japan we now have with the NASA station, we would like some in China, in the Soviet Union, in India, in the South Pacific, and this matter is being rectified, too, and also in the East Indies.

MR. NEWELL: Any others?

MR. SUOMI: I might add Central Africa.

MR. POMERANTZ: I would like to say Amen to that and to have as much coverage in the equatorial region as possible.

MR. ROSEN: What is in Central Africa?

MR. SUOMI: Nairobi.

MR. ROSEN: Any more questions?

QUESTION: I have one more question for Dr. O'Brien.

Presumably, Doctor, it is conceivable, isn't it, that during the remaining active lifetime of the satellite that these sporadic bursts that have occurred between the inner and outer layers, you get more records on, them and is it conceivable that you will get something on the nature of these things to tie in with this question of whether or not they might be protons from the sun?

MR. O'BRIEN: Yes, indeed. This is one of the very great advantages of the long life of Explorer VII, because these are relatively rare phenomena and by, of course, extending the lifetime we hope to get more examples of them to help clear up some of the problems.

QUESTION: I do not know whether this is a question for Dr. Newell or for Dr. O'Brien. But when I was out at the University of Iowa maybe six or nine months ago, we had a lot of the early Vanguard and Explorer data but it was being processed manually with only a handful of people.

Here we spend great sums of money to go out and make these measurements and get the data and then we save a few pennies by not buying the modern data, handling and analyzing equipment on the market.

Have any steps been taken to speed up the reduction of the useful data from the raw data from the satellites.

MR. NEWELL: Let me take a crack at an answer to this.

This is a very serious problem because after all the data are the harvest of all your activity and also because it is a very costly phase; it is remarkable how expensive this can get when you sit down and begin to add up the dollars.

For this reason NASA has a committee right now of people concerned with this business working out a data acquisition handling and use plan which will involve the questions of the modern equipment, computers, and so forth, and their availability to the scientists to get this work done.

This committee does not have an easy job and we do not expect them to have a full answer for perhaps as much as half a year, so we expect to pick up partial answers for the individual satellites as we go along and we are already picking up answers for the satellites that are in orbits.

MR. ROSEN: Incidentally, Phil, just the cost of tape alone is running at $5,000 a month.

QUESTION: I appreciate these things are costly but the end result of all the expense is useful data and it would be a shame if buried somewhere in these tapes is some data that is of value but we would not discover it for another three years.

QUESTION: Is this invitation for participation that is going out to scientists throughout the world, is that for participation for Explorer VII only?

MR.ROSEN: Let'sfinish this one here.

MR. THOMPSON: I would like tomake a statement about this automatic reduction of satellite data. When one realizes the fact that you are pressing transmitter power, information rates, you have a lot of ground interference which you cannot control and all in all you normally get back signals which are, when reduced to paper are actually put on magnetic tape, are not of a quality that you can reduce with equipment.

We are in luck when we have passes of any duration that are suitable for automatic reduction, so --

QUESTION: I do not want to labor the point but girls were sitting out there with rulers, not even using these elementary tapes, pushing a button, having a punched card automatically fall out. Out there were one or two girls sitting there, miles and miles of tape and they were analyzing it, I guess, at about a foot per minute rate.

MR. THOMPSON: Our experience has been keeping this machine running that you punch the button on and which is just about as time-consuming as some of the reduction.

MR. NEWELL: It is not an easy problem and the answer is going to be individually tailored to individual situations and in some cases, in fact in many cases you will continue to have people with rulers and miles of tape to pick out spots and look at them.

In other cases you will use the machines when these work out best.

MR. ROSEN: I think the SUI representative would like to --

MR. O'BRIEN: No; my comment has already been stated.

MR. LA GOW: I would like to ask Dr. Suomi to make a statement to be used. He is planning to use a machine.

MR. SUOMI: The experimenter at the beginning of the experiment is naturally inquisitive about what's going on and you usually try to do things by hand because this seems the fastest. Then you reach the stage where you know you have to put it down because the data is piling up on you. Well, we have at Wisconsin a program to utilize the 704 which the Mid-Western Universities Research Association has there. The apparatus is set up to accept data at high speeds, approximately 512 times the regular play-back speed. This noise which has been mentioned is definitely a problem; it's almost as bad as trying to talk against this banging radiator we have had all afternoon. But there are some tapes, a large number, actually, which are very clear and these can be processed automatically. Moreover, this is a very challenging situation to engineers, and so on to eliminate the noise where it is possible, and this is being worked upon. We are very close, but we are not quite there. Perhaps in another month or so.

MR. ROSEN: Well, gentlemen, --

QUESTION: Would you answer that question about whether or not this invitation includes only the Explorer VII or all satellites that we have up there that are still operating?

MR. NEWELL: This is only Explorer VII; we have the telemetering codes from this satellite.

MR. ROSEN: We have ten more months of expected life to go.

MR. NEWELL: This same consideration will undoubtedly come up in connection with every satellite we put up, but will have to be handled in connection with individual satellites. Sometimes the whole problem of reducing the data and interpreting them properly without getting errors to creep in will mean that this has to stick with the scientists who designed the experiment and put it up.

QUESTION: Thank you very much.

MR. ROSEN: Thank you very much.

(Whereupon, at 3:30 p.m. the press conference was concluded.)